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ART. XLII.—*The Outlet of Lake Bonneville;* by G. K. GILBERT.

SOME of the readers of the Journal will remember that the name "Lake Bonneville" has been applied to a great body of water which formerly covered the desert basins of Utah. Its most conspicuous vestiges are its shore lines, and from them it is known that the ancient water surface was more than ten times as great as that of Great Salt Lake, and the ancient water level was about one thousand feet above the modern.

It was early surmised that the ancient lake was freshened by overflow, but the point of discharge was for a long time undiscovered, and it may be said to be still in controversy. The present paper takes up the subject of the outlet where it was left nearly two years ago.

In this Journal for April, 1878, the writer maintained that the point of outflow was Red Rock Pass, Idaho, at the north end of Cache Valley; that the discharging stream descended through Marsh Valley and thence continuously to the Pacific Ocean; and that, flowing over soft material at first, it gradually excavated at the Pass a channel more than three hundred feet deep, and lowered the lake level by the same amount. In the June number of the Journal, Dr. A. C. Peale controverted my conclusion, declaring, first, that the original altitude of Red Rock Pass was considerably below the highest level of Lake Bonneville; second, that the ancient shore line exists in Marsh Valley at the north of the pass, just as in Cache Valley at the south; and finally, that the real point of discharge, when the water stood at the Bonneville level, was about forty-five miles north of Red Rock Pass.

Dr. Peale treats, in the same article, a question of priority and other matters of purely personal interest, and his remarks thereon invite reply, but I have a strong distaste for personal controversy, and I am confident that the readers of the Journal—not even excepting Dr. Peale—will cheerfully excuse me if I refrain. I shall therefore confine myself to the question of outlet.

Within a few months I have revisited Marsh Valley and Red Rock Pass, and have examined the lower cañon of the Portneuf. My observations do not serve to diminish materially the disparity between Dr. Peale's conclusions and my own, but they throw light on some of our contradictions in matters of simple observation. These contradictions depend in part upon a misinterpretation of certain terraces, and will be better understood after a brief enumeration of the characters by which terraces of diverse origin may be distinguished.

The term *terrace* is applied in topography to a level surface, or one of very gentle slope, limited on one side by a surface which *descends* at a greater angle, and usually limited on the other by a surface which *ascends* at a greater angle. Where the limiting slope is steep it is called a *scarp*. A scarp may stand above a terrace, rising from its inner edge and facing toward it, or it may lie below it, falling from its outer edge and facing from it. Most terraces are margined by scarps on one edge or the other, and some on both.

Terraces are produced in at least five different ways, namely: by differential erosion, by streams, by waves, by differential deposition, and by displacement.

*Terraces by Differential Erosion* arise wherever a series of dissimilar strata lying nearly horizontal are subjected to rapid erosion. The soft strata are destroyed more rapidly than the hard, and the latter, where they overlie soft beds, are undermined so as to break off by vertical fracture. A stair-like system of terraces and scarps is thus formed, in which each terrace marks the outcrop of a soft rock and each scarp the outcrop of a hard one.

These terraces are distinguished from all others by the constant relation of form to stratigraphic structure.

A *Stream terrace* is produced whenever a stream, which has for a long time, flowed at one level, is induced by changed conditions to excavate a new channel at a lower level. While flowing at the upper level the water forms a broad flood plain. By the subsequent excavation this plain is in part destroyed, and what remains becomes a terrace. A scarp of even height separates the terrace from the new channel of the stream or from a new flood plain.

It is characteristic of stream terraces that they slope in the direction of the stream that carved them.

*Wave terraces* are found on the shores of all bodies of water. The action of waves, combined with that of currents, carves away the land on all salients. The direct action is confined to a few feet above and below the water-level, but whatever is undermined by the waves is thrown down by gravity and brought within their reach. The results of the erosion are, first, a broad terrace lying under shallow water and sloping gently from the shore; and, second, a sea-cliff or scarp springing from the water's edge.

It is one of the chief characteristics of a wave terrace that its upper edge, where it joins the scarp, follows a water line. In the case of Lake Bonneville the water has long since disappeared from the terraces, but the horizontality remains as a conspicuous feature—subject only to slight modification by orographic displacement.

*Delta terraces* are produced by differential deposition. Where a stream enters a lake or other body of deep water, the chief part of the detritus it transports is deposited at its mouth. The effect of this is to extend its bed on the side toward the deep water. As the mouth protrudes a deposition takes place along the bed, so as to maintain a constant declivity of channel, and soon the bed is so raised that the water finds a lower way at one side and leaves it. By a repetition of this process the mouth of the stream is shifted from point to point, and the delta is made to encroach on the lake along its whole face. The form eventually produced is that of a terrace sloping gently down to the shore and there limited by a submerged scarp. The declivity of the scarp depends on the coarseness of the material deposited. The declivity of the terrace is identical with that of the stream which formed it. The contour separating the terrace from the scarp is a curve convex toward the lake.

A chief characteristic of a delta terrace is that its lower edge, where it joins the scarp, follows a water line and is horizontal. In the delta terraces of Lake Bonneville the horizontality remains though the water has disappeared.

*Terraces by displacement* are usually produced by faulting. A fault traversing a surface of gentle slope drops down the portion at one side or lifts the portion at the other, and separates the two by a scarp. The scarp is the only feature added to the topography, but by its addition the raised portion of the slope becomes a terrace.

Whatever is characteristic of the terrace by displacement pertains to the scarp. A *fault scarp* usually holds an even height for long distances. It maintains an even, often a

straight, course across the country, ascending and descending slopes without change of direction. It is only by accident that it is ever horizontal.

Wave terraces and delta terraces are associated phenomena of shore lines. They agree in the essential character of horizontality, and by that are distinguished from all other terraces. They have no such relation to rock structure as have terraces by differential erosion. Their scarps are not of even height like those of stream terraces. Their course across the country has the sinuosity of a contour, and not the directness of a line of orographic displacement.

Returning now to the consideration of the outlet of Lake Bonneville, let us glance a moment at the general features of the vicinity. Cache Valley and Marsh Valley are parts of the same trough-like depression, separating two mountain ranges. The general trend of the trough and of the ranges is a little west of north. Marsh Valley lies to the north of Cache Valley. It is thirty-five miles long and has a maximum width of fourteen miles. Cache Valley is fifty-five miles in length and eighteen in width. At the north end of Marsh Valley the trough is ended by the junction or close approximation of the bordering ranges. At the south end of Cache Valley it is interrupted by a low cross range. At Red Rock Pass, where the two valleys adjoin, the trough is restricted by spurs from the two ranges, and a few protruding crags show that there is a low cross ridge of limestone buried by more recent deposits. On the west side of Cache Valley there is a low interval in the bounding range—so low that the Bonneville flood overtopped it and made Cache Valley a bay of the great lake. At this point Bear River has cut a narrow gorge, through which the drainage of the entire valley finds its way to the basin of Great Salt Lake. Bear River and several other streams enter Cache Valley from the east by cañons traversing the bounding range. In like manner the Portneuf cuts through the eastern range to enter Marsh Valley, and then passes from it at its extreme northwestern angle by a deep cut valley of erosion, opening to the great plain of the Snake River.

According to Dr. Peale, the Bonneville flood filled Cache and Marsh Valleys to the same level. Red Rock Pass was only a point of stricture of the lake, and the outlet was at the north. He mentions terraces in Marsh Valley and gives their altitudes, impliedly regarding them as shore terraces. According to my own observations, Cache Valley was occupied by Lake Bonneville, and Marsh Valley was not so occupied, but was traversed from south to north by the stream which overflowed at the north end of Cache Valley. There are no shore terraces nor

other lake phenomena of the Bonneville epoch in Marsh Valley.

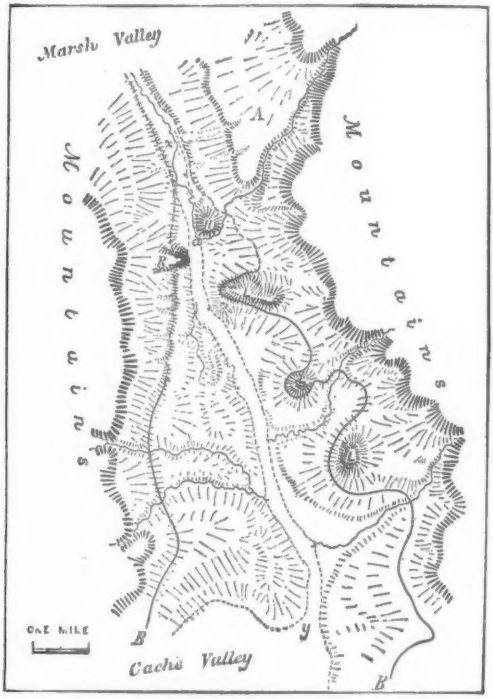
In revisiting Marsh Valley I traversed it from end to end and made careful search for ancient shore terraces. The most favorable position from which to detect such vestiges is one which brings the eye in the same horizontal plane with them. The apparent distortion which arises from perspective is thus avoided, and all the little modifications of surface wrought by the waves, being brought into one straight line across the landscape, appeal to the eye by their *ensemble*. The most favorable light for the observation is one which throws the scarps into shadow or deep shade, and thus contrasts them with the adjoining terrace faces. In examining Marsh Valley I gave particular attention to the selection of stations and of light, and I considered my inspection of no part of the margin complete until I had viewed it from the proper height and in a favorable light. I ascended to a level corresponding to that of the Bonneville beach at ten different points, and the series of views thus obtained omitted no portion of the valley. I saw stream terraces and displacement terraces of considerable magnitude and a few inconspicuous terraces due to unequal erosion, but no wave terrace and no delta terrace.

If Marsh Valley had really been filled by the lake, as Cache Valley was, there could be no difficulty in finding evidence of the fact. The water would have been eight or ten miles broad and 400 feet deep, and the waves raised by the wind on a bay of such dimensions would leave most conspicuous monuments of their force. Arms of Lake Bonneville two or three miles in width and less than 100 feet in depth have elsewhere been traced out by means of their distinct and well-characterized shore lines, and this under conditions of slope, etc., similar to those existing in Marsh Valley.

But if Marsh Valley contained no lake, of what nature are the terraces adduced by Dr. Peale in support of his conclusions? The question is easier proposed than answered, but I think a partial explanation can be given.

The first locality he mentions is "two miles north of Red Rock Pass, on the east side of the valley." At this place there is a conspicuous stream terrace at about the right altitude to personate a delta terrace of the Bonneville series. Its surface is part of an alluvial cone formed by Marsh Creek before the Bonneville epoch, and its scarp is one wall of the channel worn by the outflowing river during that epoch. The river pared away the margin of the sloping plain spread by the creek and left the remainder as a terrace. The edge of this terrace is, at its highest point, 50 feet higher than the nearest trace of the Bonneville beach, two miles away; and it descends northward

until it passes below the level. On top of the terrace and about half a mile back (east) from its edge, there is a second scarp from 10 to 20 feet in height, and due to displacement. This fades out to the northward and at the same time descends with the general slope. Dr. Peale's observation probably refers to one or the other of these scarps, and in either case fails to take account of the lack of horizontality.




His second locality is "six miles west of Red Rock Pass." The only terraces I found at that point are stream terraces, sloping so conspicuously to the N.E. and N.W. that it is hard to believe they were mistaken for beaches. A few miles beyond there is a fault scarp with a much stronger semblance, and where this crosses the old freight road which ascends to Malade Pass it has very nearly the altitude appropriate for the Bonneville Beach. Its true character, however, cannot be mistaken when it is critically examined.

His third locality, "twenty-six miles from Red Rock Pass on the west side of the valley," was not identified.

Passing now from the subject of the general position of the outlet to the question of its precise location at the time of the commencement of the outflow, I find myself able to make a concession to Dr. Peale. I at first supposed that the divide originally lay between Red Rock and Hunt's Butte, just as it lies to-day, and I so described it. A careful reëxamination of the locality has convinced me that I was in error, and has led me to assign it a position two miles north of Red Rock. Dr. Peale placed it about forty-five miles north of Red Rock, so that my new determination is nearer to his than my old was.

With the aid of the accompanying map I hope to make the chief topographic features of the locality, and the history of the outflow, clear to the reader.

At the point of greatest orographic constriction, that is, where the mountains bordering the general trough, approach nearest to each other, there is evidence of a connecting ridge of limestone. This ridge is so low that only a few peaks of its serrated crest project through the superficial deposits. To it belong Red Rock (R), Hunt's Butte (H) and the hills marked L, L, L. Just north of the ridge Marsh Creek issues from the mountains at the east. It flows southwestward for three miles, makes a sharp turn about Hunt's Butte, and then flows northward to Marsh Valley, occupying the old river bed all the way from Hunt's Butte. It is the largest stream of the vicinity of the pass, and has thrown so much debris into the river bed that it has determined the modern divide at the point of its accession. It is contained by high banks all the way from its mountain cañon to Hunt's Butte and has no freedom to shift its course over its own alluvion until it enters the river bed. Before the Bonneville epoch,—before the river bed was cut, the creek had dug no deep channel outside the mountain cañon and was free to roam at will. It distributed its debris equally in all directions, building a fan-shaped alluvial plain (A) with its apex at the mouth of the cañon. All parts of this plain radiated with equal slope from its apex, and its surface was part of a low-crowned cone. In brief it was an *alluvial cone.* It extended westward until it met the foot-slopes of a spur of the western range and that point of meeting ($x$), which was about two miles north of Red Rock, was the divide between Cache and Marsh Valleys. When the brimming waters of Lake Bonneville were discharged across it, a river channel was slowly excavated, and the margin of the alluvial cone was cut away. The edge of the surviving slope is fifty feet higher than the Bonneville beach at Hunt's Butte and therefore fifty feet higher than the original divide. As the channel of outflow deepened by wear, the lake level was lowered and its shore retreated southward. The head of the outflowing stream fol-

lowed the retreating shore until, at the Provo stage of Lake Bonneville—the lowest stage of outflow,—the point of outlet was at the edge of what is called Round Valley Marsh (*y*), nine miles south of Red Rock. When the lake dried away, the divide was left at that place, but Marsh Creek has resumed control of it by constructing an alluvial cone in the old river bed at Hunt's Butte (*a*) and building it higher than the level of the last discharge.

The Bonneville water line, so far as it survives, is represented by the heavy lines B, B. Its restored continuation and the restored banks of the outflowing river are represented by heavy broken lines. The margins of the water at its lowest, or Provo stage, when the point of outlet was at *y*, is shown by lighter broken lines.

While Marsh Creek was building its ancient alluvial cone, it ran in turn over all parts of it. At times it passed south of the divide and flowed to the Great Basin. At other times it passed north of the divide and flowed to the Portneuf river. When the outflow of the lake was initiated and the water level began to be lowered by the deepening of the channel of outflow it chanced that the creek was running over the southern part of its cone, and discharging into the lake at Hunt's Butte. The lowering of its point of discharge caused the creek to cut away its bed instead of building it up and it ceased to shift its channel. As the river bed grew deeper, so did the channel of the creek, and the latter now flows several hundred feet below the top of its old cone. When, by the building of its new cone in the abandoned river bed, the creek recovered possession of the divide, it resumed its ancient practice of discharging alternately to the north and to the south. To-day it is a tributary of the Portneuf. The next freshet may clog its channel with debris and divert it to the drainage of Bear river.

After the publication of my former article, I learned that the outlet had been independently discovered by my friend, Mr. Gilbert Thompson, and I am glad of this opportunity to give him credit. Mr. Thompson is not a professed geologist, but he is an expert topographer, and his close study of the natural forms, which it is his work to delineate, has more than once led to observations valuable to the geologists with whom he has been associated. I quote the following from his letter dated April 10, 1878:

"Thanks for your brochure, 'the Ancient Outlet of Great Salt Lake.' The past season I was along the northern limits of the ancient lake, between 111° and 112° 22′ 30″ and was absolutely ignorant of your examination in 1876 and its results. . . . . I was very much interested in the general subject of its limits and also of its outlet. . . . . Toward

the last of the season, as I surveyed from the north the road through Red Rock Pass, after noting the remarkable topographical features of Marsh Creek, and keeping a close run of the profile as given by the aneroid, I was delighted at Red Rock to see unmistakable evidences of the ancient outlet of Great Salt Lake. . . . . Thus you may have the gratification of knowing of an independent and entirely unbiased verification of your determination of this point; and it is nowhere else in the limits I have mentioned."

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ART. XLIII.—*The Chemical and Geological Relations of the Atmosphere;** by T. STERRY HUNT, LL.D., F.R.S.

QUESTIONS concerning the condition of the terrestrial atmosphere in former periods of the earth's history, and its geological relations, have occupied the attention of naturalists, physicists and chemists. Bronguiart long since suggested that the abundant vegetation of the Coal-period indicated the existence of a large proportion of carbonic acid in the air at that time. Ebelmen, however, appears to have been the first to clearly understand the great geological significance of the atmosphere, and in his two remarkable memoirs on the decomposition of rocks, published in the Annales des Mines in 1845 and 1847,† treated the subject in its atmospheric relations with much research and philosophic breadth. Starting from the chemical changes of crystalline silicate rocks, he considered both the conversion of feldspars into kaolin, and the decay of protoxide-silicates, such as amphibole and olivine. The sub-aerial decomposition of the feldspars had already been shown by Berthier to result in the separation, in a soluble form, of the protoxide-bases, together with a portion of silica, from an insoluble aluminous silicate of definite composition. The analyses of Ebelmen now established the fact that the protoxide-silicates just mentioned, lose, under similar conditions, the whole of their lime and magnesia, and nearly the whole of their silica, leaving little behind save the higher oxides resulting from the fixation of atmospheric oxygen by the ferrous and manganous oxides of the silicates;

* A summary of the views presented in this memoir was given at Dublin, in August, 1878, before the British Association for the Advancement of Science. An abstract thereof appeared in the Proceedings, and will be found in *Nature* for August 29, 1878 (vol. xviii, p. 475.) The principal conclusions of the memoir are also embodied in a communication made by the author to the French Academy of Sciences, and published in the Comptes Rendus of September 23, 1878 (vol. lxxxvii, page 452.) They will also be found set forth in the preface to a second edition of the writer's Chemical and Geological Essays (pages ix–xix) published in the spring of the same year.

† 4th Series, vols. vii and xiii. These memoirs will also be found in the Receuil des Trav. Scient. de M. Ebelmen; Paris, 1855, vol. ii, pp. 1–79.

the soluble bases being in all cases removed by atmospheric waters in the form of carbonates. Such a decomposition of these silicates shows that the removal of silica in soluble form does not depend on the intervention of alkalies.

The atmosphere of our earth at a pressure of 760 millimeters has a weight of 10,333 kilograms to the square meter, of which the oxygen equals 2,376, and the carbonic dioxide (if we take Boussingault and Lévy's determination of four and a half parts in 10,000 parts by weight) 4·64* kilograms. The alkali of 100 parts of orthoclase would require for its neutralization 7·8 parts of carbonic dioxide, so that a cubic meter of this silicate, of specific gravity, 2·5, would, by the calculation of Ebelmen, fix, in the process of decay, 195 kilograms of the gas. From this it results that a layer of orthoclase over the earth of 0·0238 meter, or one of less than 1·0 meter over one-fortieth of its surface, would, in its decomposition, absorb the whole amount of this gas now present in the atmosphere. Ebelmen further calculated that the formation of a layer of kaolin by this process, 500 meters in thickness, would require an amount of carbonic dioxide equal to many times the weight of the present atmosphere.

We have repeated and extended these calculations, with revised equivalent weights, and with the following results: A cubic meter of orthoclase, with a density of 2·5, and containing theoretically 16·9 per cent. of potash, equivalent to 7·89 of carbonic dioxide, would absorb in kaolinization 197·3 kilograms of this gas, while a cubic meter of albite of density 2·6, containing 11·8 of soda, equivalent to 8·37 of carbonic dioxide, would require not less than 217·6 kilograms of the same. The figure of 195 kilograms, adopted by Ebelmen, was thus below the truth, and we may, in view of the considerable proportion of soda-feldspar in the oldest crystalline rocks, conveniently assume 200 kilograms as the amount of carbonic dioxide required to unite with the alkali from a cubic metre of orthoclase or albite, and form therewith a neutral carbonate.

In such a decomposition, 100 parts of orthoclase give theoretically about 46·5 parts of kaolin, so that 1·0 meter in thickness of orthoclase of the above density should yield 0·447 meter of kaolin of density 2·6. If we assume this process to have consumed for a cubic meter or 2500 kilograms of orthoclase, 200 of carbonic dioxide, we find that a layer of 51·66 meters of orthoclase, or its equivalent of quartzo-feldspathic rock, in undergoing the same change, would absorb 10,333 kilograms of this gas, equal to the entire weight of the present atmospheric column, and would yield a layer of pure kaolin 23·7 meters in thickness. The production of a stratum of

* This, by an error in Ebelmen's memoir, is given as only 1·24 kilograms.

kaolin 500 meters in thickness over the whole surface of the globe, would thus require an amount of carbonic dioxide equal to more than twenty-one times the entire weight of our present atmosphere.

The absorption of this gas in the decay of silicates like hornblende, pyroxene and olivine is far greater. If we assume for convenience a hornblende containing 20·0 per cent of magnesia, and 14·0 of lime, with a density of 3·0 (which figures are not above the average) we find that it will require 33·0 per cent, or in round numbers one-third its weight of carbonic dioxide to convert these two bases into neutral carbonates; so that a meter-cube of hornblende, weighing 3000 kilograms, would consume not less than 1000 kilograms of carbonic dioxide. In other terms, the decay of $10\frac{1}{3}$ meters of such hornblende (or its equivalent in hornblendic rock) would absorb 10,333 kilograms, or a whole atmosphere of this gas, being five times as much as is taken up in the kaolinization of the same volume of orthoclase.

The hornblendes in question are seldom without several hundredths of iron as ferrous oxide, which is peroxidized in the process of decay, and, with a little silica, is the chief insoluble residue in the case of non-aluminous hornblendes. In this connection we revert to a farther calculation by Ebelmen, who pointed out that the conversion of 21,357 kilograms of ferrous oxide into 23,750 kilograms of ferric oxide would consume the whole of the 2,373 kilograms of oxygen contained in the present atmosphere; so that if we suppose the existence over the whole earth of 1,000 meters of sediments derived from the decay of crystalline rocks, and containing only one per cent of ferric oxide thus formed, this amount would equal 25,000 kilograms per square meter of surface, requiring for its production from ferrous oxide the absorption of a quantity of oxygen more than equal to that now contained in our atmosphere.

Ebelmen, at the same time, referred to the well-known deoxidation of carbonic dioxide by growing vegetation, and also to the reduction, by decaying organic matters, of sulphates to sulphids, with reproduction of carbonic dioxide, through which the generation of metallic sulphids in nature gives to the atmosphere, in union with carbon, a portion of the oxygen previously combined with sulphur and with the metals.

The following calculations may serve to bring still more fully before us the great geological significance of these atmospheric changes. The weight of a layer of pure carbon, with a density of 1·25 and a thickness of 0·7 meter, would require for its conversion into carbonic dioxide the whole of the oxygen of our present atmosphere. The separation of such an amount

of carbon by the process of vegetable growth must therefore have liberated the same volume of oxygen Again, a stratum of carbonate of lime of specific gravity 2·7, covering the earth with a thickness of 8·69 metres, (or one of dolomite of sp. gr. 2·85, and 7·58 meters thick) would contain an amount of carbonic dioxide equal in weight to the present atmosphere.*

It was in view of these processes that Ebelmen declared, in 1845, that "the decomposition and the reproduction of certain mineral species very abundant on the surface of the globe corresponds to important modifications in the composition of the atmosphere." He farther said, "Many circumstances tend to prove that in ancient geological periods the atmosphere was denser, and more rich in carbonic acid, and perhaps in oxygen, than at present. To a greater weight of the atmospheric envelop would correspond a stronger condensation of the solar heat, and atmospheric phenomena of a much greater intensity."† Similar conclusions with regard to the physical relations of a denser primeval atmosphere were subsequently announced by the late Edwin B. Hunt, in an essay on Terrestrial Thermotics, presented to the American Association for the Advancement of Science, in 1849, and published in its Proceedings for that year, page 135.

We may get a clearer notion of the problem before us by inquiring into the probable amounts of carbonic dioxide which have, in past ages, been abstracted from the atmosphere. In a communication to the British Association for the Advancement of Science, in 1877,‡ Mr. J. L. Mott concludes, as the result of calculations, that the average amount of unoxidized carbon to a square mile of the earth's crust cannot be less, and is probably many times greater than 3,000,000 tons; while a layer of 0·7 meters of carbon of density 1·25, (about that of coal) which we have calculated to be equal to the total atmospheric oxygen, would weigh only about 2,200,000 tons to the square mile. Mr. Mott rightly argues that the presence in the atmosphere of so great an amount of carbon in the form of dioxide would imply a condition of things incompatible with the existence of animal life, and at the same time concludes that its deoxidation would yield an excessive amount of oxygen. He is hence led to assume the existence in the earth of a constant amount of carbon, which is subject to an annual subterranean oxidation equal to the amount of carbon annually removed by vegetation; the source of the original amount of carbon being, in his hypothesis, left unexplained.

* T. Sterry Hunt on the Primeval Atmosphere, Proc. Amer. Assoc. Adv. Science, 1866, and Can. Naturalist, II, iii, 118.

† Ann. des Mines, IV, vii, 65; also Receuil des Trav. Scient. de M. Ebelmen, vol. ii, p. 55.

‡ Nature, vol. xvi, p. 406.

While some have imagined an inorganic origin to the carbon found in the form of graphite, and even to petroleum and to coal, sound reasoning is, we think, on the side of those who, starting from the conception of an originally oxidized globe, see no evidence of any process of deoxidation therein which does not, directly or indirectly, depend upon vegetable life, and hence assign an organic origin to all carbons and hydro-carbons. When we take into account the vast amounts of these, from the graphite of Eozoic times to the coals, lignites and petroleums of the Tertiary, we can scarcely doubt that the total amount of carbon which has been reduced from carbonic dioxide is equal to many times the equivalent of the oxygen now present in the atmosphere. Whether the great excess of oxygen thus liberated may perhaps have been absorbed in the production of ferric oxide, as above indicated, is a part of the problem before us.

It may here be noted that in addition to the fossil carbonaceous bodies already mentioned, the rocky strata of the earth include great thicknesses of pyroschists, which are argillaceous sediments more or less impregnated with hydro-carbonaceous matters allied to coal in composition. To give a single example, Newberry estimates the proportion of such matters diffused through the 300 or 400 feet of Devonian black shales which underlie the eastern half of Ohio, to equal fifteen per cent, and to be equivalent to a layer of coal fifty feet in thickness over the whole area.*

In this connection it must be considered that the chemical composition of the various hydrocarbonaceous fossil substances implies a deoxidation not only of carbonic dioxide but of water. The amount of liberated oxygen from the latter would equal, for the different coals and asphalts, from one-eighth to one-fourth, and for the petroleums, one-half of that set free in the deoxidation of the carbon which these hydrocarbonaceous bodies contain.

The amount of carbon removed from the atmosphere in a deoxidized form by vegetation is, however, small when compared with that which has been absorbed during the decomposition of silicates, and is now fixed as insoluble carbonates, chiefly in the form of limestones and dolomites. That both the alkaline carbonates liberated in the decay of feldspars, and the magnesian carbonate set free in like manner from magnesian silicates, must decompose the chlorid of calcium contained in the primitive ocean, thereby giving rise to alkaline and magnesian chlorides on the one hand, and to carbonate of lime on the other, is a consequence which seems to have escaped Ebelmen, and was pointed out by the present

* Geology of Ohio, vol. i, page 162.

writer in 1858. In 1862, however, there was opened a sealed packet which had been in 1844 deposited by Cordier with the French Academy of Sciences, and was found to contain views as to the origin of limestones and of sea-salt similar to those just stated.* Thus, in the present state of our knowledge we conclude that all carbonates of lime, whether directly formed by the decay of calcareous silicates, or indirectly through the intervention of carbonates of magnesia or alkalies, derive their carbonic dioxide from the atmosphere. The same must be said for the dolomites, magnesites and siderites.

We have already shown that a weight of carbonic dioxide equal to more than twenty-one times that of our present atmosphere would be absorbed in the production from orthoclase of a layer of kaolin extending over the earth's surface with a thickness of 500 meters, an amount which evidently represents but a small proportion of the results of feldspathic decay in the sedimentary strata of the globe. The aluminous silicates in the oldest crystalline rocks occur in the forms of feldspars and related species, and are, so to speak, saturated with alkalies or with lime. It is only in more recent formations, that we find aluminous silicates either free or with reduced amounts of alkali, as in the argillites and clays, in micaceous minerals like muscovite, margarodite, damourite and pyrophyllite, and in kyanite, fibrolite and andalusite, all of which we regard as derived indirectly from the more ancient feldspars.†

It has been shown that the disengagement of the carbonic dioxide from a layer of limestone covering the earth's surface with a thickness of 8·69 meters, would double the weight of the atmosphere. The existence of vast formations of limestone and dolomite, often many hundred meters in thickness, throughout all geological periods, will, it is believed, justify the conclusion that the carbonates of the earth's crust are equal to a continuous layer of limestone 869 meters thick, and probably to more than double this amount. From this it would follow that the earth contains, fixed in the form of carbonates, a quantity of carbonic dioxide, which, if liberated in a gaseous form, would be equal in weight to one hundred if not to two hundred atmospheres like the present. A considerable portion

* Hunt, Chem. and Geol. Essays, pp. 2 and 20.

† These considerations, and their stratigraphical bearings, first set forth in 1863 (Chem. and Geol. Essays, pp. 27 and 28), will be found further developed in the writer's report on Azoic Rocks, 2d Geol. Survey of Penn., 1878, p. 210. It is a question how far the origin of the various crystalline aluminous silicates named above is to be sought in a process of diagenesis in ordinary aqueous sediments holding the ruins of more or less completely decayed feldspars. Other aluminous rock-forming silicates, such as chlorites and magnesian micas, are however connected, through aluminiferous amphiboles, with the non-aluminous magnesian silicates, and to all these various magnesian minerals, as maintained in a previous paper in this volume (History of some pre-Cambrian Rocks, etc., page 270), a very different origin must be assigned.

of this was doubtless absorbed at an early period in the history of our globe, since the limestones of the Eozoic are of great thickness, and those of more recent times have been in part formed by the solution and re-deposition of portions of these older limestones.

The question now arises, whence came this enormous volume of carbonic dioxide which, since the dawn of life on our planet, has been fixed in the form of carbon and carbonates? The presence of even a small proportion of it at any one time in the terrestrial atmosphere is evidently incompatible with the existence of vegetable and animal life, and it may be added that the pressure of a column of this gas less than the minimum of 100 atmospheres which we have supposed, would suffice, at ordinary temperatures, for its partial liquefaction; the tension of liquid carbonic dioxide at 30°·7 C. being, according to Mareska and Donny, but eighty atmospheres. We are therefore forced to the conclusion that this gas was gradually supplied from a source either within the earth or beyond our atmosphere.

The difficulties of this problem were not overlooked by Ebelmen, though he apparently failed to recognize their full weight. He takes care to remark: "I do not pretend that this immense proportion of carbonic acid ever made part, at any one time, of the terrestrial atmosphere. . . . . I see in volcanic phenomena the principal agent which restores to the atmosphere the carbonic acid which the decomposition of rocks removes from it." He then inquires whether the carbonic acid (carbonic dioxide) evolved from the earth's interior, comes from the decomposition of carbonates at great depths and high temperatures by reactions with silicious matters, or whether we may imagine, with Elie de Beaumont, the existence of an immense reservoir of carbonic acid dissolved in the supposed liquid interior of the earth as oxygen is held in fused litharge or in molten silver. In either case, remarks Ebelmen, the cessation of volcanic phenomena would be followed by the removal from the atmosphere of the last traces of carbonic acid, a process which would entail the extinction of all vegetable and animal life.

Of these two suggested sources of the terrestrial carbonic dioxide, a little reflection will show that although the first is doubtless a true one, and will serve to account for that which is so often disengaged from the earth, both in volcanic and non-volcanic regions (having a similar origin to the chlorhydric, sulphuric and boric acids evolved under analogous conditions—namely, the decomposition of saline compounds of aqueous origin),* it by no means meets the requirements of the problem.

* Hunt, Chem. and Geol. Essays, pp. 8 and 111.

As preceding calculations have shown, it is not a question of a small amount of carbonic dioxide alternately removed from our atmosphere by sub-aerial reactions and restored to it by subterranean processes, but of a vast quantity of this gas which, at one time or another, has existed in the terrestrial atmosphere, but is now removed from the aerial circulation and locked up in the form of carbonates.

As regards the second source of carbonic dioxide, suggested by Ebelmen after Elie de Beaumont, it is, unlike the last, purely hypothetical. That the globe has a molten interior is, in the present state of our knowledge of terrestrial physics, very improbable, and if such exists, the notion that it intervenes directly in volcanic phenomena is still more so. The suggestion that such a molten interior might hold dissolved a great volume of carbonic dioxide appears, moreover, to be inconsistent with what we know of the behavior of furnace-slags, which, though formed in atmospheres highly charged with this gas, do not, as shown by their behavior in cooling, hold it in solution. The tendencies of modern geological thought and investigation, it may be said, lead to the conclusion that the seat of volcanic phenomena is to be found in sedimentary strata,* and that although the earth's interior intervenes as a source of heat, the carbonic dioxide disengaged from its crust is derived, as in the first hypothesis mentioned by Ebelmen, from the decomposition of carbonates previously generated by sub-aerial reactions.

The problem still before us is then to find the source of the vast amount of carbonic dioxide continuously supplied to the atmosphere throughout the geologic ages, and as continuously removed therefrom, and fixed in the form of carbonaceous matters and limestones. We have shown reasons for rejecting the theory which would derive this supply either from the earth's interior or from its own primal atmosphere, and must therefore look for it to an extra-terrestrial source. The new hypothesis, which we here advance, starts with the assumption that our atmosphere is not primarily terrestrial but cosmical, and that the air, together with the water surrounding our earth, (whether in a liquid or a vaporous state) belongs to a continuous elastic medium which, extending throughout the interstellary spaces, is condensed around attracting bodies in amounts proportional to their mass and temperature. This universal atmosphere (if the expression may be permitted,) would then exist in its most attenuated form in the regions fartherest distant from these centers of attraction; while any change in the gaseous envelope of any globe, whether by the absorption or condensation, or by the disengagement of any gas or vapor

* Ibid, pp. 59–67.

would, by the laws of diffusion and static equilibrium, be felt everywhere throughout the universe.

The precipitation of water at the surface of a cooling globe, and its chemical or mechanical fixation there, would thus diminish the proportion of gaseous water throughout all space. The oxygen liberated in the growth of terrestrial vegetation would be shared with the remotest spheres, while the condensation of carbonic dioxide at the surface of our own or any other planet, would not only bring in a supply of this gas from the atmospheres of other bodies, but by reducing the total amount of it, would diminish, *pro tanto*, the barometric pressure at the surface of this and of all other worlds.

The hypothesis here advanced is not wholly new. Sir William R. Grove, in 1842, suggested that the medium of light and heat may be "a universally diffused matter," and subsequently, in 1843, in his celebrated Essay on the Correlation of Physical Forces, in the chapter on Light, concludes, with regard to the atmospheres of the sun and planets, that there is no reason why these atmospheres "should not be, with reference to each other, in a state of equilibrium. Ether, which term we may apply to the highly attenuated matter existing in the interplanetary spaces, being an expansion of some or all of these atmospheres, or of the more volatile portions of them, would thus furnish matter for the transmission of the modes of motion which we call light, heat, etc., and *possibly minute portions of these atmospheres may, by gradual accretions and subtractions, pass from planet to planet, forming a link of material communication between the distant monads of the universe.*" Subsequently, in his address as President of the British Association for the Advancement of Science, in 1866, Grove further suggested that this diffused matter might become a source of solar heat, inasmuch as the sun "may condense gaseous matter as it travels in space, and so heat may be produced."

This bold speculation of a universally diffused matter, constituting an interstellary medium, though thus repeatedly insisted upon by Grove, has passed almost unnoticed. It seems to have been unknown to Mr. W. Mattieu Williams, who in 1870 published his very ingenious work entitled "The Fuel of the Sun,"* which is based on a similar conception, without citing in support of it the high authority of Grove. The solar heat, according to Williams, is maintained by the sun's condensation of the attenuated matter everywhere encountered by that body in its motion through interstellary space. The irregular movements impressed upon the sun by the varying attractions of the planets, stirring up and intermingling the

* See also Williams on The Radiometer and its Lessons, Quart. Jour. Science, Oct., 1876.

different strata of the solar atmosphere, and producing the great perturbations therein of which the telescope affords evidence—are, in his hypothesis, the efficient agents in this process. The diffused matter or ether, which is the recipient of the heat-radiations of the universe, is thereby drawn into the depths of the solar mass; expelling thence the previously condensed and thermally-exhausted ether, it becomes compressed, and gives up its heat, to be, in turn, itself driven out in a rarified and cooled state, and to absorb a fresh supply of heat, which he supposes to be in this way taken up by the ether, and again concentrated and redistributed by the suns of the universe. (Loc. cit., chap. v.)

Neither Grove nor Williams has considered the hypothesis of an interstellary medium in its geological relations. Dr. P. Martin Duncan, however, in his address as President of the Geological Society of London, in May, 1877, without noticing the priority of Grove, has adopted it from Williams,* but instead of supposing, with these, that the atmospheres of all bodies are in equilibrium, conceives the sun, in virtue of its greater mass, to be slowly attracting to itself the earth's terrestrial envelope. He thence proceeds to deduce therefrom important geological considerations, maintaining that from the greater height of the terrestrial atmosphere which, according to this view, must have prevailed in former ages, there would have resulted a higher temperature at the earth's surface, more aqueous vapor, and a more equable climate. From a more abundant precipitation would also follow greater sub-aerial denudation, while the formation of ice, though it might occur in elevated regions, would be impossible at or near the sea-level.

The correctness of all these deductions by Duncan from the condition of a denser terrestrial atmosphere appears to be indisputable, and, as we shall endeavor to show in the sequel, they are in harmony with the geological record. But, while admitting that changes in the earth's atmosphere conducing to such results have taken place, we maintain, in accordance with the principles already laid down, that these changes have not been due to solar attraction and absorption, but to the chemical and mechanical processes going on at the surface of the earth and other bodies in space, whereby the atmospheric elements are condensed in the forms of liquid and solid water, or fixed as hydrates, oxides, carbonates and hydrocarbonaceous matters.

The changes which have thus been produced in the terrestrial atmosphere are, by our hypothesis, reduced in amount by being shared with other worlds, and the consequences which

* It is due to my friends, Mr. Williams and Dr. Duncan, to say that they have both, in conversation, informed me that they were ignorant of the priority of Sir William Grove. The conception appears to have been original and independent in the mind of Mr. Williams.

Ebelmen, and others after him, have deduced with regard to the temperature of the earth's surface in former geological periods, would seem, at first sight, to be invalidated. Tyndall, however, in 1861, from a consideration of the great power of absorbing heat possessed alike by aqueous vapor and by certain gases, such as carbonic dioxide, and the consequent effects of small quantities of these in the atmosphere on terrestrial radiation, and thus on climate, was led to remark, "it is not therefore necessary to assume alterations in the density and height of the atmosphere to account for different amounts of heat being preserved to the earth in different times; a slight change in its variable constituents may have produced all the mutations of climate which the researches of geologists reveal."* Thus, although the amount of carbonic dioxide which, in past geological ages, has been, by chemical processes at the surface of our own and other worlds, abstracted from the universal medium, may not have sufficed to diminish by more than a small fraction the barometric pressure at the earth's surface, this change would still meet all the requirements of geological history, so far as temperature and climate are concerned. From this point of view, the suggestion of Tyndall assumes a weight and a significance not hitherto suspected.

We have thus briefly endeavored to show how the hypothesis of a universal atmosphere serves to explain certain chemical and physical facts in the history of our globe. To discuss it in all its bearings would require a volume. The climatic influences of a denser terrestrial atmosphere, or one of greater absorptive power than the present, have been indicated by Ebelmen, E. B. Hunt and Duncan, and, as we have seen, the gradual changes in the composition of the atmosphere imply a slow progressive diminution of the mean annual temperature of the earth's surface. This conclusion is in contradiction with the hypothesis of secular oscillations of the earth's temperature, due to astronomical causes, and giving rise to successive periods characterized by general glaciation, and leads us to interrogate on this point the geological record. We may inquire (1) whether, since the appearance of terrestrial vegetation, the mean annual temperature of the earth has ever been less, and (2) whether it has ever been greater than at present. It is clear from paleontological evidence that a very warm climate prevailed over the arctic regions during the Carboniferous, Triassic, Jurassic, and Lower Cretaceous periods, and that the refrigeration apparent in the Upper Cretaceous gradually augmented up to the Pliocene, the cold of which has continued till now, subject to certain variations in its distribution which are

* Tyndall, Bakerian lecture for 1861; L., E. & D. Phil. Mag., Oct., 1861, and Hunt, Chem. and Geol. Essays, pp. 42 and 46.

readily accounted for by changed geographical conditions. Such changes of sea and land are, however, inadequate to explain the elevated temperature which, according to the observations of Nordenskiöld, prevailed in the Carboniferous age, when the arctic climate permitted the development, over a great area of land, of a vegetation not unlike the Carboniferous flora of the inter-tropical regions. It is not easy to conceive that, with an atmosphere like that of the present time, any geographical conditions could maintain during the long polar winter the mild climate required for such a vegetation, even in insular regions, and still less over a continental area within the polar circle.

We are thus led to the conclusion that geographical changes, though adequate to explain the *greater refrigeration* of certain areas since the beginning of Pliocene time, are not sufficient to account for the *warmer climates* of previous ages, and to find the explanation of these in the different relations of the earlier atmosphere alike to solar and to terrestrial heat.

It is, however, obvious that, with such an atmosphere as we have supposed, the more elevated portions of the earth's surface might, as is now the case in inter-tropical lands, be lifted into regions where glaciation was possible, while a warm climate prevailed everywhere at the sea-level. Neither the glacial periods of more recent times, nor those of remoter geological ages, of which evidence is not wanting, necessarily depend upon any diminution in the earth's mean annual temperature at the sea-level. Glacial periods are, in this view, as has been well said by J. F. Campbell, not celestial, but local and terrestrial,* while, on the contrary, the warmer polar climates of Paleozoic and Mesozoic times are to be regarded as evidence of a generally elevated temperature at the earth's surface depending on atmospheric conditions, as already set forth.

In a note in the Comptes Rendus of the French Academy of Sciences, for Oct. 7, 1878, criticising my previous one of Sept. 23 "Sur les relations géologiques de l'atmosphere," already referred to at the beginning of this paper, Mr. Stanislas Meunier has argued in favor of the terrestrial origin of the atmospheric carbonic dioxide, the source of which he supposes to be a subterranean oxidation of a primitive store of carbon, a view which seems unsupported by any facts or analogies in nature. He opposes to the hypothesis which I have advocated, the fact of the absence of an atmosphere from the moon, while he asserts the existence of an abundant one around both Mercury and Venus. The evidences of such an atmosphere around the latter planet are well known, but the observations of recent

* Campbell on Glacial Periods; Quart. Jour. Geol. Soc., 1879, vol. xxxv, p. 98.

astronomers leave it doubtful, on the contrary, whether Mercury possesses a perceptible one, while, as regards our satellite, the conclusion, as stated by Newcomb, is that the lunar atmosphere, if it exist, is not equal to more than one four-hundredth that of the earth.

A little reflection will, however, show that the absence of any apparent atmosphere from the moon in no way militates against our hypothesis, since a completely refrigerated globe, such as our satellite must probably be, would long since have absorbed mechanically into its interstices, its share of the universal gaseous medium. It was many years since pointed out by Sæmann* that, as a consequence of the progressive refrigeration of our planet, the ocean and the air which surround it must one day disappear from its surface. The total volume of our atmosphere, at the density which it has at the sea-level, is, according to his calculation, less than four thousandths of that of the earth, the volume of the ocean being very much less. There is no known mass of cooled rock which has not a greater porosity than is represented by these figures, so that the conclusion seems inevitable that, with the complete refrigeration of the earth which must come in the course of ages, its atmosphere, following the ocean, will have so completely sunk into the pores of the cooled mass that its tension at the surface would be very small. Such a condition of things, Sæmann supposes to have been already attained in our satellite, a view which may be, with equal probability, extended to Mercury.

The hypothesis that interstellary space is filled with an attenuated matter which, in a more condensed form, constitutes the atmosphere and the waters of our own and other worlds, which we have already discussed in some of its chemical and geological bearings, assumes a new interest in connection with recent speculations as to evolution in the stellar universe. In considering the increasing chemical complexity revealed by the spectroscope in passing from nebulæ to white, yellow and red stars, Prof. F. W. Clarke, of Cincinnati, was led in 1873† to suggest the possibility of a generation of the higher from simpler forms of matter by a process of cosmical chemistry. A similar view was a few months later advanced by Mr. Lockyer, who reiterated and enforced these suggestions, showing that the chemical elements make their appearance in the cooling stars in the order of their vapor-densities—and moreover connected these considerations with the conjectures of Dumas as to the probably compound nature of the so-called

* Sur l'unité des phénomènes géologiques dans le système planétaire du soleil, Bull. Soc. Géol. de Fr., 1860–61, vol. xviii, p. 322, translated by the present writer for this Journal, II, xxxiii, p. 36.

† Popular Science Monthly, Jan., 1873, vol. ii, p. 32.

elements.* Mr. Lockyer has since extended this inquiry by his ingenious and beautiful spectroscopic studies, the results of which are embodied in his "Discussion of the Working Hypothesis that the so-called Elements are Compound Bodies," communicated to the Royal Society, Dec. 12, 1878.† In his first note, of 1873 (which is embodied in the later paper) he suggested that we see in the stars evidences of a *celestial dissociation* under the influence of intense heat, which, continuing the work of our furnaces, would break up the metalloids, and leave only the metallic elements of low equivalent weight which are found in the hottest stars. In his later memoir he further suggests that as there may be no superior limit to temperature, so of dissociation there may be no end.

With these may be compared the views enunciated by the present writer in a lecture before the Royal Institution, May 31, 1867, wherein, discussing the problems of stellar chemistry, he declared that the "dissociation of elements by intense heat is a principle of universal application," and with regard to the chemical elements, that their "further dissociation in stellar or nebulous masses may give us evidence of matter still more elemental than that revealed by the experiments of the laboratory, where we can only conjecture the compound nature of many of the so-called elementary substances."‡ In 1874, while discussing the speculations of Dumas, Clarke and Lockyer, he further suggested that the green line in the spectrum of the solar corona, which had been supposed to indicate a hitherto unknown element, may be a "more elemental form of matter, which, though not seen in the nebulæ, is liberated by the intense heat of the solar sphere, and may possibly correspond to the primary matter conjectured by Dumas, having an equivalent weight one-fourth that of hydrogen."§ Regarding this supposed element in the solar atmosphere, Prof. C. A. Young remarks that it must be of excessive tenuity, "a near relative, so far as gravity is concerned, to the luminiferous ether, and to the *Urstoff* of the German speculators."‖ In this connection it should be mentioned that Hinrichs, in 1866, put forth an argument¶ in favor of the existence of such a primitive matter or *Urstoff* from a consideration of the wavelengths in the spectra of the various elements.**

* Comptes Rendus, Nov. 3, 1873.

† This Journal, III, xvii, 93–116; and farther, Clarke, Science News, Feb. 15, 1879, page 114.

‡ Reprinted from Proc. Royal Institution in Chem. and Geol. Essays, p. 37.

§ A Century's Progress in Theoretical Chemistry, by T. S. Hunt, being an address delivered on the Centennial of Chemistry, at Northumberland, Penn., July 31, 1874; Amer. Chemist, vol. v, pp. 46–51, and Pop. Science Monthly, vii, 420.

‖ This Journal, II, xlii, 350–368. ¶ This Journal, III, i, 319.

** Since these pages were in type my attention has been called to a paper read before the Literary and Historical Society of Quebec in January, 1870, by James

Lavoisier long since suggested that hydrogen, nitrogen and oxygen are, with heat and light, the simpler forms of matter from which all others are derived, and when it is considered that the first two of these are the only elements of which we have yet any certain evidence in the nebulæ, it will be seen that the speculation of Lavoisier is really an anticipation of that view to which spectroscopic study has led the chemists of to-day. The three elements named by him are those which, in the forms of air and watery vapor, make up nine hundred and ninety-nine thousandths of the atmosphere which, in accordance with our hypothesis, constitutes the interstellary medium. It was in view of all these considerations that the writer in 1874 ventured to say that "the nebulæ and their resultant worlds may be evolved by a process of chemical condensation from this universal atmosphere; to which they would sustain a relation somewhat analogous to that of clouds and rain to the aqueous vapor around us."* Such a speculation, which seeks for a source of the nebulous matter itself, is perhaps a legitimate extension of the nebular hypothesis.

Montreal, Feb. 14, 1880.

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ART. XLIV.—*On the Archæan Rocks of the Wahsatch Mountains;* by ARCHIBALD GEIKIE, F.R.S., Director of the Geological Survey of Scotland, and Murchison Professor of Geology in the University of Edinburgh.

THE complete physical break between the crystalline schists and the overlying sedimentary groups in the Rocky Mountains and other ranges of the west has been clearly described by Gilbert, King, Hayden, Emmons and other writers. It is quite possible, however, that in these regions there may have been subsequent protrusions of granite and accompanying metamorphism; so that we ought not to decide that a mass is necessarily Archæan merely because it consists of schists and granites. Yet I am not sure that this assumption has not to a certain

Douglas, Jr., then President of the Society, and one of the Canadian Expedition to observe the total solar eclipse of August 7, 1869. Therein, while discussing the spectroscopic observations made during the eclipse, he refers to those of Professor Young, who had suggested a comparison between certain lines in the spectrum of the solar corona, and those observed by Winlock in that of the aurora borealis. With regard to these lines, Mr. Douglas then adds, "May they not therefore belong to some unknown element;—a gas, lighter than hydrogen, which, like the hypothetical ether, fills space?" To this he adds the suggestion that electricity both "in the auroral light of our own heavens, and the corona of the sun may render this hypothetical gas luminous." *Trans. Lit. and Hist. Soc. of Quebec*, New Series, part 7, p. 82.

* A Century's Progress, etc., cited above; also Chem. and Geol. Essays, preface to 2d Ed., p. xix.

extent influenced the observations of some of the able geologists to whom we owe our acquaintance with these western ranges. In a recent visit to the Wahsatch Mountains I was strongly inclined to doubt the correctness of the interpretation of the central and so-called Archæan portion of the chain given in the Reports of the Geological Exploration of the 40th Parallel. And I am disposed to offer the results of my visit for the consideration of those who are much more familiar than myself with the geology of that part of the United States.

Let me begin by expressing my unqualified admiration of the geological prowess shown by Mr. Clarence King and his associates, Messrs. Hague and Emmons, in their great survey. Having traveled over many hundreds of miles with their works in my hands I can bear testimony to their remarkably clear and accurate delineation of the broad geological features of the country. It is perhaps somewhat presumptuous in one who has only made a single journey through the region to offer any criticism of work which occupied years of continuous toil. Yet I am sure these writers, with the feeling of true scientific brotherhood, will themselves be the most desirous to give me a hearing, and will not require any assurance that my remarks are called forth by no spirit of fault-finding.

According to the Reports of the Exploration of the 40th Parallel, the Wahsatch Mountains consist of a central core of Archæan rocks, composed partly of granites and partly of various quartzite, schists, and other crystalline masses. These rocks are represented as having formed an island in the Paleozoic sea; and Mr. King asserts that the island must have presented to the west an almost precipitous face of 30,000 feet, or upwards of 5½ miles—an altitude exceeding that of any modern mountain chain.* Round this lofty Archæan island the whole of the Paleozoic and Mesozoic sediments are said to have been deposited to a depth of from 30,000 to 40,000 feet, in one continuous uninterrupted series. Subsequent terrestrial movements, acting along the line of the original island, have upraised the surrounding sedimentary masses, and the ancient crystalline rocks have once more been revealed by denudation.

Now the fact of the existence of a cliff more than 5½ miles high would require to be established by very carefully collected and convincing evidence. It was with very considerable curiosity therefore that I paid a visit to the Cottonwood district, where the evidence was said to be most complete. I must frankly own that I failed to observe any grounds on which the assertion appeared to me to be warranted. One would naturally expect that if a mass of strata 30,000 feet thick had been laid down against a steep slope of land, its component beds

* Geol. Exploration of 40th Parallel, vol. i, p. 124.

ought to be full of fragments of that land. Each marginal belt, representing an old shore-line, should be more or less conglomeratic; at least, there ought to be occasional zones of conglomerate, just as at the present day, we have local gravel beaches on our shores. But I could find no trace of pebbles. It would of course be presumptuous in me to assert that they do not exist; but they are not mentioned by Mr. King, nor by Messrs. Hague and Emmons, and yet, as their evidence would be so important, we can hardly suppose that these writers observed them and made no reference to the fact.

But not only have no pebbles of the Cottonwood granite been recorded as occurring in the overlying Paleozoic rocks, it is admitted that these rocks become metamorphosed as they approach the granite. The natural inference to be drawn from these facts, one might suppose, would be that the granite is later in date than the rocks overlying it. Mr. King admits that the granite had been undoubtedly the center of local metamorphism, but this change he regards as "strictly mechanical and not to be mistaken for the caustic phenomena of a chemically energetic intrusion." (p. 45). How he would discriminate between a mechanical and chemical cause producing precisely the same ultimate effect he does not explain. Had he not been firmly convinced that all the granite must be Archæan he would hardly, I venture to think, have penned that sentence. Two pages farther on he admits that round the granite mass the Carboniferous limestones have been invaded by igneous dikes, and these rocks (named granite-porphyry by Zirkel), he asserts to be "middle-age porphyries, not to be confounded with the Archæan crystalline rocks." (p. 49). But why should they be "middle age," or rather on what grounds are we to separate them from the neighboring granite? Not a single reason is given save the obvious one that when a geologist has made up his mind that a granite is Archæan he cannot of course admit that it sends out ramifications into overlying Paleozoic rocks. Yet the natural tendency of any unbiased observer must, I should think, be to connect these surrounding dikes with the main granite mass inside. Curiously enough, Mr. King himself, in another passage, admits that they are "in all probability a dependence of the granite." (p. 46). But surely the occurrence of intrusive dikes twenty feet broad penetrating limestones that have been converted into marbles is something more than a "strictly mechanical" phenomenon. One argument given by Mr. King for the antiquity of the granite is that it does not send out dikes into the overlying rocks. (p. 48). But, as he himself is no doubt well aware, the veins or dikes which penetrate the rocks around a granite boss are not always themselves granite. They very commonly take the form of his "granite-porphyry."

I submit, therefore, that the facts taken by themselves and without reference to any preconceived opinion or theory, are these: 1st. A large mass of granite* on the Wahsatch Mountains ascends with an oblique or transgressive boundary line from certain schists and quartzites across the Paleozoic series up into the Upper Carboniferous horizons. 2d. The rocks as they approach the granite manifest increasing metamorphism; the limestones pass into white granular marble, and other rocks have assumed the character of schist. 3d. Among the Carboniferous limestones and other rocks around the main mass of granite, there occur, in different places, dikes or veins of granite-porphyry, like those usually met with in a similar position. The conclusion which I would draw from these facts is that the granite is intrusive, and is later in date than the Upper Carboniferous rocks which have been metamorphosed in contact with it. Mr. King speaks of there being possibly two granites, one metamorphic and one intrusive, but both of Archæan date. I could observe no reason for making any subdivision in the granite mass. The somewhat foliated arrangement in certain portions of the granite is not uncommon toward the periphery and even within the central portions of intrusive bosses.

The section across the Wahsatch range, placed below map No. III (east half) of the Geological Exploration of the 40th Parallel, seems to me to bear the strongest evidence against Mr. King's own reading of the structure of the mountains. If the granite there shown as underlying the highly tilted Paleozoic rocks be regarded as anterior in date to these rocks, as, in fact, the land surface against which they were laid down, the section, I submit, involves a series of physical impossibilities, or at least of such glaring improbabilities as to demand full and incontrovertible proof in its support. For, in the first place, it requires us to believe that the cliff against which the Paleozoic sediments were deposited, must have been *at least twelve miles* high! that being the horizontal length of the platform of granite into which the rocks dip. In the next place it necessitates the admission that this stupendous precipice was subsequently *turned over on its back*, carrying with it the adhering later rocks. In the third place, it demands an amount of denudation to which there would be no parallel anywhere in the region, for the highly tilted strata must then have been worn down till nothing but a cake of them was left upon the granite. If, however, this granite be younger than the overlying rocks, the section expresses sufficiently well the structure of the ground. That the latter is the natural interpretation of

* There can be little doubt that though partially interrupted at the surface by overlying formations it is really all one granite mass.

the section will, I feel sure, be admitted by any impartial geologist in whose hands the map is placed.

I venture to put forward these views with diffidence, and because it appears to me to be a matter of great importance not merely in regard to the geological structure of the West, but to questions in dynamical and physiographical geology, that the true structure of the Archæan nucleus of the Wahsatch Mountains should be correctly interpreted. Mr. King, in his great memoir, has treated that area as a kind of type, and has based upon it much of his speculation regarding the form of the Archæan land, and the nature and effects of subsequent fractures of the rocky crust. I confess that it was with considerable incredulity that I read in his interesting chapters reiterated assertions that the Archæan land was so stupendously mountainous, that some of its peaks rose more than 5½ miles into the air, and remained above water during the whole of Paleozoic and Mesozoic time. I asked myself how much loftier and broader these mountains must really have been at first to have been able to outlast such a vast period of denudation. For the dimensions assigned by Mr. King must on his own showing be a minimum, reckoned after all these ages of ceaseless waste. But if I am correct in regarding the Wahsatch granite as of post-Carboniferous date, then we are relieved from the uncomfortable incubus of these primeval mountains. We are not required to believe in the existence of a cliff 5½ miles high, which maintained its position and steepness during the greater part of all geological time. And we are spared the necessity of a colossal fracture of 30,000 feet on the west side of the Wahsatch Mountains. The view which I am inclined to adopt regarding the structure of this range differs from that proposed by Mr. King; and perhaps I may be permitted to communicate it on another occasion in the pages of this Journal.

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ART. XLV.—*Analyses of some Apatites containing Manganese;* by SAMUEL L. PENFIELD. (Contributions from the Sheffield Laboratory of Yale College, No. LIX.)

IN their description of the mineral locality at Branchville, Conn. (this Journal, July, 1878), Messrs. Brush and Dana mention the occurrence of a green manganiferous apatite accompanying the other manganese minerals. Apatite occurs there of many shades of color, from those which are white and transparent to those which are dark green, and still others of a bluish shade. The green varieties occur in flat crystalline

masses imbedded in feldspar; occasionally the form of the short prism is distinct. The white variety is usually in crystals; these crystals are short prisms combined with the pyramidal and rough pinacoid planes. The prismatic planes have a fibrous appearance, although they are polished and very smooth, and the pinacoids are found on close inspection to give numerous reflections from their surfaces, when looked at obliquely and turned, showing that the crystals are made up of bundles of minute hexagonal prisms of the same length, each with a small pyramidal termination. There have also been found there a few transparent and very highly modified crystals. All the varieties examined contain manganese, as the following analyses will show.

I was led by the discovery of manganese in the apatite from Branchville, to examine the same mineral occurring at Franklin Furnace, New Jersey; manganese was also found to be present in it. This apatite occurs in crystals of a light apple-green color imbedded in calcite, from which they are readily separated.

The material employed in the four analyses, given below, was as follows:—Analysis 1 was made of a dark green variety from Branchville, the darkest that was found. It has a vitreous luster, appearing black by reflected light, but a beautiful dark green by transmitted light. Only clear transparent fragments were accepted.

Analysis 2 was made in the Sheffield Laboratory by Mr. Frederick P. Dewey, of a green variety from Branchville, lighter in color than the one just described. Analysis 3 was of the white crystallized variety from Branchville. Great care was taken to select only the crystals which have been described before as having the rough pinacoid planes. Analysis 4 was made of the crystallized variety from Franklin Furnace, N. J. The whole amount employed was taken from one large crystal. It readily separated from the calcite in which it was imbedded, and although the analysis shows the presence of carbonic acid, it was from no external admixture of calcite.

*No.* 1. *Specific gravity*, 3·39.

| | I. | II. | Mean. | Ratio. | | |
|---|---|---|---|---|---|---|
| $P_2O_5$ | 41·60 | 41·66 | 41·63 | 2·93 | | 1 |
| $Fe_2O_3$ | ·77 | | ·77 | | | |
| CaO | 40·31 | | 40·31 | ·720 | ·869 | 2·97 |
| MnO | 10·59 | | 10·59 | ·149 | | |
| Ca | | | 3·29 | ·082 | | ·28 |
| F | 3·20 | 3·04 | 3·12 | ·164 | | ·56 |
| Cl | ·03 | | ·03 | ·000 | | |
| | | | 99·74 | | | |

*No.* 2, *by Mr. F. P. Dewey.*

| | I. | II. | Mean. | | Ratio. | |
|---|---|---|---|---|---|---|
| $P_2O_5$ | 40·94 | 40·99 | 40·96 | ·288 | | 1 |
| $Al_2O_3$ | ·52 | ·47 | ·50 | | | |
| $Fe_2O_3$ | ·09 | ·08 | ·08 | | | |
| CaO | 47·79 | 47·95 | 47·87 | ·855 | ·890 | 3·09 |
| MnO | 2·50 | 2·47 | 2·48 | ·035 | | |
| Ca | | | 4·04 | ·101 | | ·35 |
| F | 3·84 | 3·84 | 3·84 | ·202 | | ·70 |
| Insoluble | ·06 | ·05 | ·06 | ·000 | | |
| | | | 99·83 | | | |

*No.* 3. *Specific gravity*, 3·144.

| | I. | II. | Mean. | | Ratio. | |
|---|---|---|---|---|---|---|
| $P_2O_5$ | 41·47 | 41·47 | 41·47 | ·292 | | 1 |
| $Fe_2O_3$ | ·22 | ·22 | ·22 | | | |
| CaO | 49·10 | 49·13 | 49·12 | ·877 | ·905 | 3·01 |
| MnO | 1·98 | 1·94 | 1·96 | ·028 | | |
| Ca | | | 2·88 | ·072 | | ·25 |
| F | 2·68 | | 2·68 | ·141 | ·144 | ·49 |
| Cl | ·10 | | ·10 | ·003 | | |
| Insoluble | 1·50 | | 1·50 | | | |
| | | | 99·93 | | | |

*No.* 4. *Specific gravity*, 3·22

| | I. | II. | Mean. | | Ratio. | |
|---|---|---|---|---|---|---|
| $P_2O_5$ | 39·58 | 39·59 | 39·59 | ·279 | | 1 |
| $Al_2O_3$ | ·56 | ·56 | ·56 | | | |
| $Fe_2O_3$ | ·77 | ·77 | ·77 | | | |
| CaO | 46·52 | 46·76 | 46·64 | ·833 | ·852 | 3·05 |
| MnO | 1·38 | 1·31 | 1·35 | ·019 | | |
| ZnO | ·03 | ·03 | ·03 | | | |
| Ca | | | 3·57 | ·089 | | ·32 |
| F | 3·40 | 3·34 | 3·37 | ·177 | ·178 | ·64 |
| Cl | ·04 | | ·04 | | | |
| $CaCO_3$ | 2·82 | | 2·82 | ·001 | | |
| $H_2O$ | ·52 | | ·52 | | | |
| | | | 99·26 | | | |

The above ratios coincide very nearly with that required by the accepted formula of the species, viz: 1 : 3 : 0·33 : 0·67.

These analyses are the first that show the presence of notable quantities of manganese replacing calcium in apatites. It is also to be noted that these apatites are essentially fluor-apatites containing only a trace of chlorine.

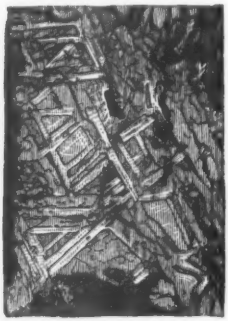
In closing, I wish to express my thanks to Professor George J. Brush, who has kindly provided me with the material for carrying on this investigation, and to Mr. Frederick P. Dewey, whose analysis I have quoted.

ART. XLVI.—*An account of the finding of a new Meteorite in Cleberne County, Alabama;* by W. E. HIDDEN.

WHEN in eastern Alabama, during last autumn, carrying forward some mineralogical investigations in the interest of Mr. Thomas A. Edison, I learned from Ex-Governor W. H. Smith of Wedowee, of the existence of a supposed mass of native iron, which he believed might perhaps be a meteorite. The account which he gave me of it, in his own words, is as follows: "Sometime in 1873, while the Rev. John F. Watson was plowing on a newly cleared piece of land, near Chulafinnee, in Cleberne county, Ala., he turned up a heavy mass of metal. He supposed it to be a rich specimen of bog iron ore, which exists in considerable quantity in the vicinity, and took the mass home. Some days later he carried it to the village blacksmith to have it tested. After heating one corner in the

1.

2.



forge, a piece of about 3¼ lbs. was cut off and wrought into horseshoe nails and a point for a plow. The fact of its malleability tended to set at rest the various local theories about the origin of the mass, and it was there agreed to be a specimen of native iron. The mass was then deposited in the office of the Noble's Brothers' Iron Works at Anniston, Alabama, and remains there now unsuspected of being a meteorite, and will in all probability get into the furnace sooner or later."

Through the kindness of Governor Smith, the specimen was secured and forwarded to me. On January 21st, 1880, it was

received at Menlo Park, N. J., and was at once recognized as an iron meteorite. A letter from Mr. Watson informs me, further, that the mass was originally thickly encrusted with scales of rust of a red-brown color, and which fell off while being heated in the forge.

It now weighs 32½ lbs. (= 14·75 kg.), is somewhat triangular in shape (see cut); its three diameters being about 25$^{cm}$; its average thickness 6$^{cm}$.

An analysis by J. B. Mackintosh, E. M., shows it to be of the usual iron-nickel alloy variety, with small percentages of copper, phosphorus and carbon.* The Widmannstättian figures are well developed on this iron. They are shown in figure 2, which is of exact natural size. This meteorite is one of three discovered by the writer in the Southern States last year. Descriptions of the others will be given later.

January 29th, 1880.

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ART. XLVII.—*On the recent formation of Quartz and on Silicification in California;* by T. STERRY HUNT.

AT the meeting of the American Institute of Mining Engineers in New York, Feb. 19, 1880, Professor George W. Maynard exhibited a remarkable specimen lately obtained by him from the mines of the Gold Run Hydraulic Co. at Dutch Flat in California. It consisted of a mass of milky vitreous quartz, in which a recent fracture had disclosed an imbedded fragment, about half an inch in diameter, of the characteristic so-called *blue gravel* of the region, holding in its paste a worn and rounded piece of gold of several grains, weight. Portions of a similar blue gravel adhered closely to certain parts of the mass of quartz. Remarks were made on this specimen by Professors Silliman and Egleston, and by Dr. T. Sterry Hunt, all of whom, after examination of it, were satisfied of the correctness of the opinion expressed by Professor Maynard, that the quartz had made part of a vein formed in the auriferous gravel subsequent to the solidification of the latter.

* The analysis is now being made in duplicate, and when completed will be published in this Journal. W. E. H.

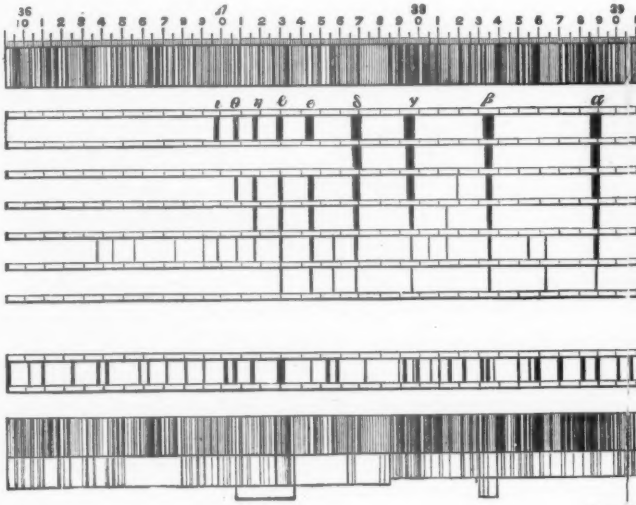
† This communication, which for want of space was excluded from the April number, had been printed and revised before the writer had seen Professor Joseph LeConte's paper on The Old River Beds of California, in the March number of this Journal, where (on pages 179–181), he has so well described the auriferous gravels here referred to, and pointed out the true relations between the blue gravel and the upper and altered portions of the deposit. As regards the process of silicification, it is not, I think, necessary to suppose the infiltration of alkaline waters from the overlying volcanic rock, to explain the solution of silica. As elsewhere pointed out by the writer (page 350 of the present volume), the removal of the silica in a soluble form from the silicates which make up a large part of the gravel itself, does not require the intervention of alkalies. T. S. H.

Dr. Hunt, in commenting upon this occurrence, remarked that it is in accordance with what we already know of the recency of some of the quartz of this region, and cited the microscopic studies of John Arthur Phillips, who has shown that a great part of the siliceous deposit from certain thermal waters of Lake County, California, and from the Steamboat Springs of Washoe County, Nevada, is of the nature of crystalline quartz. Dr. Hunt then gave an account of some observations made by him at the Blue Tent placer mine, in Nevada County, California, in 1877, showing that the process of depositing quartz is there going on in the auriferous gravel of the region, independent of thermal waters, and is connected with the sub-aerial decay of the silicates in the gravel, which is here made up in great part of the *débris* of the crystalline Huronian schists of the region, including much greenstone or diorite-rock. The gravel below the drainage-level is greenish or bluish in color, and contains disseminated pyrites, together with trunks of trees in the condition of lignite, while the feldspar and hornblende of the greenstone are undecayed. Above the drainage-level, however, these silicates are more or less decomposed, the greenstone pebbles becoming earthy in texture, rusty in color, and exfoliating, and the accompanying pyrites oxidized, while the lignite is more or less completely silicified, being sometimes converted into agatized masses, often with drusy cavities lined with quartz-crystals, and at other times only penetrated or injected with siliceous matter which has filled the pores of the exogenous wood, the vegetable tissue of which still remains, often incrusted with crystals of quartz. In still other cases, a slow subsequent decay of the latter, in coniferous woods, has left these siliceous casts in the form of bundles of fibers which have been mistaken for asbestus. The various specimens from this locality illustrate perfectly the theory of silicification of vegetable structures, set forth by the speaker in 1864,* based on his own microscopic studies conjoined with those of Göppert and of Dawson.

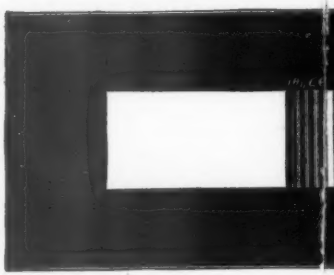
The silica by which the tissues are thus successively filled and replaced is, according to the speaker, that which is set free in a soluble form in the decay of the silicates in the gravel. The lignite in the undecomposed and unoxidized portions of this which lie below drainage-level is, as yet, unsilicified. Dr. Hunt acknowledged his obligations to Mr. D. T. Hughes, a member of the Institute of Mining Engineers, in charge of the mine in question, and a skilled and careful observer, who had called his attention to the facts just set forth.

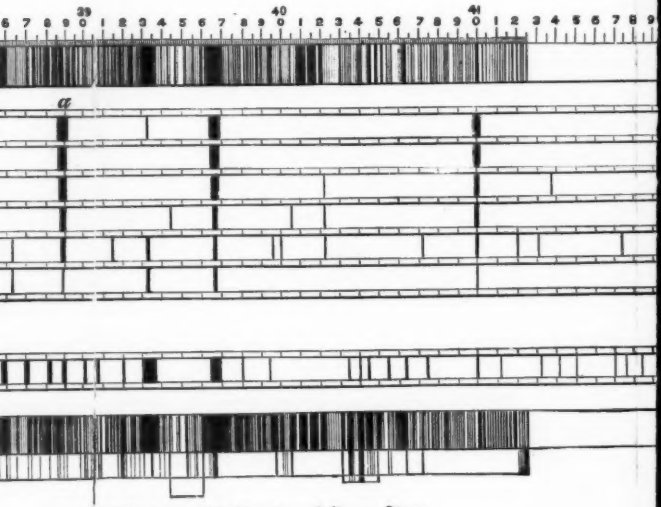
Professor W. C. Kerr stated that his recent and as yet unpublished observations on the fossil woods found in ancient gravels in North Carolina were in accordance with those described by Dr. Hunt.

* See Can. Naturalist, New Series, vol. i, p. 46; also Hunt's Chem. and Geol. Essays, p. 286.



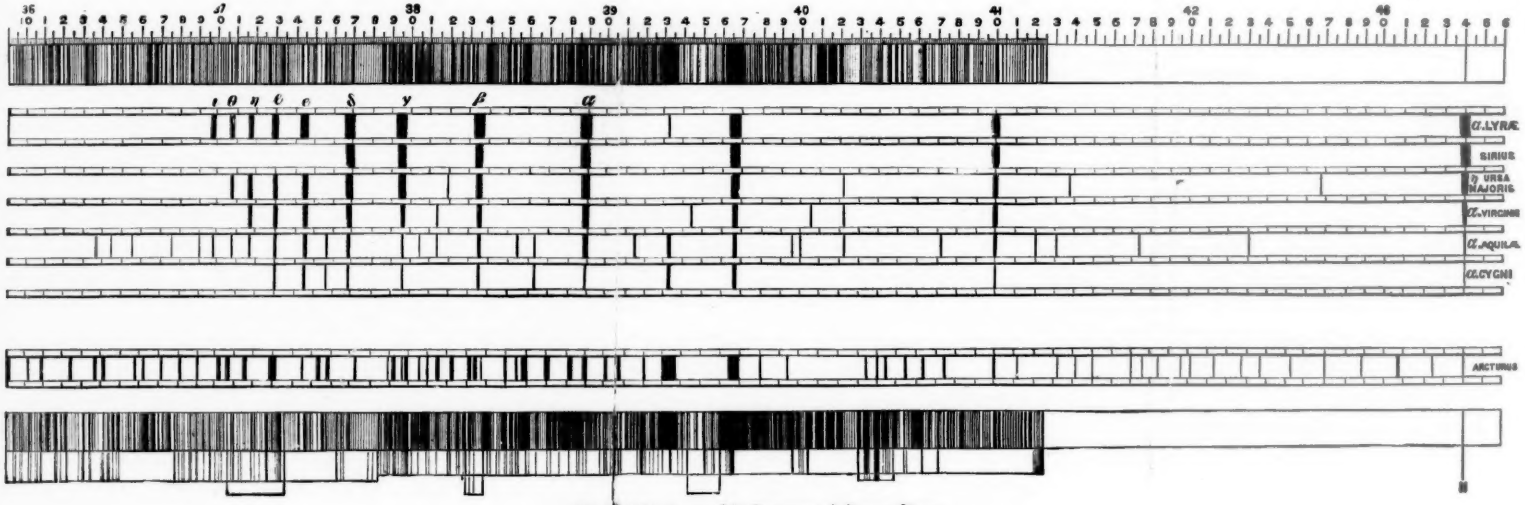
Map






Map of Photographic Spectra of Seven Stars.

Photographic Spectrum of $\alpha$ Lyræ.




Map of Photographic Spectra of Seven Stars.

Photographic Spectrum of *a* Lyræ.

## ART. XLVIII.—*On the Photographic Spectra of Stars.**

THE author presented, in December, 1876, a preliminary note on the subject of this paper, together with a diagram of the spectrum of Vega compared with that of the sun.

The author refers to a paper by Dr. William Allen Miller and himself in 1864, in which they describe an early attempt to photograph the spectra of stars.

Other investigations prevented the author from resuming this line of research until 1875, when a more perfect driving clock, by Grubb, enabled him to take up this work with greater prospect of success.

The author describes the special apparatus and the methods of working which have been employed.

In consequence of the very limited amount of light received from the stars, it was of great importance not to spread out the spectrum to a greater extent than was necessary for a sufficient separation of the principal lines of the spectrum. The spectrum apparatus finally adopted consists of one prism of Iceland spar and lenses of quartz. The length of the spectrum taken with this apparatus is about half an inch, from G to O in the ultra-violet. The definition is so good that in photographs of the solar spectrum at least seven lines can be counted between H and K.

Though there is considerable loss of light in the employment of a slit, still, for the great advantage which it affords in obtaining spectra of comparison, a narrow slit one-three-hundred-and-fiftieth ($\frac{1}{350}$) of an inch in width was always employed.

This slit is provided with two shutters. By means of these through one-half of the slit a solar or other spectrum may be taken on the same plate for comparison, and for the determination of the lines in position in the spectrum. This apparatus was adapted to a Cassegrain reflector with a metallic speculum of 18 inches aperture. The small mirror was removed and the slit of the spectrum apparatus placed at the principal focus of the mirror. A simple but perfectly successful method was adopted by which the image of a star could be brought exactly upon the slit, and retained there during the whole time of exposure, sometimes for more than one hour, by a system of continuous supervision, and instant control by hand when necessary.

Various photographic methods were tried, but the great sensitiveness which may be given to gelatine plates, together with the special advantages under long exposure of dry plates led finally to the exclusive adoption of this method.

* Abstract of paper by W. Huggins, D.C.L., LL.D., F.R.S., read before the Royal Society, December 18, 1879, with additions by the author.—*Nature*, Jan. 22.

The photographs were examined and the lines measured by means of a micrometer attached to a microscope of low power. These measures were reduced to wave-lengths by the help of solar and terrestrial spectra, use being made of M. Cornu's map of the ultra-violet part of the spectrum, and of M. Mascart's determination of the wave-lengths of the lines of cadmium.

Photographs have been obtained of the stars Sirius, Vega, *a* Cygni, *a* Virginis, $\eta$ Ursæ Majoris, *a* Aquilæ, Arcturus, $\beta$ Pegasi, Betelgeux, Capella, *a* Herculis, Rigel, and *a* Pegasi. Also of the planets Jupiter, Venus, and Mars, and of different small areas of the moon.

The spectra of Sirius, Vega, *a* Cygni, *a* Virginis, $\eta$ Ursæ Majoris, *a* Aquilæ and Arcturus are laid down in the map on the scale of M. Cornu's map of the ultra-violet part of the solar spectrum.

The stellar spectra extend from about G to O in the ultra-violet.

Six of these spectra belong to stars of the white class. In 1864 the author pointed out the features in common in the visible spectra of these stars. These photographs present a remarkable typical spectrum consisting of twelve strong lines (seven only of these were given in the preliminary note in 1876). The least refrangible of these is coincident with the hydrogen line ($\gamma$) near G. The second with *h*, also a line of hydrogen. The third with H. . K, if present at all, is thin and inconspicuous.*

These lines, H and K, are coincident with lines in the calcium spectrum, and are usually attributed to the vapor of this substance. Now there is another pair of strong lines in the spectrum of calcium, which in M. Cornu's map have the wave-lengths 3736·5 and 3705·5. There are no strong lines in the white stars coincident with these lines. A glance at the map will show how remarkable is the arrangement in position of these twelve typical lines. They form a great group in which the distance between any two adjacent lines is less as the refrangibility increases. It is at once suggested that they are connected with each other and represent probably one substance, and two at least belong to hydrogen.

It should be stated that the continuous spectrum extends in the photographs beyond S, but no lines can be detected beyond the twelfth line at $\lambda$ 3699. For the sake of convenience of reference the author distinguishes these lines by the letters

* The author refers to Mr. Lockyer's paper, *Proceed. R. S.*, No. 168, 1876, in which he suggested that photographs of the spectra of the brighter stars might show modifications of this character of the lines of the calcium spectrum, and that such modifications would confirm his views on the dissociation of this substance. Reference is also made to *Proceedings R. S.* December, 1878. fig. 1, where Mr. Lockyer gives a fuller statement of his views on this and other points in connection with different classes of spectra of the stars.

of the Greek alphabet in the order of refrangibility, beginning with the first line beyond K of the solar spectrum. The wave-lengths of these lines are as follows:—

| Hydrogen near | | Hydrogen near | |
|---|---|---|---|
| 1. G | 4340 | 7. $\delta$ | 3767·5 |
| 2. $h$ | 4101 | 8. $\varepsilon$ | 3745 |
| 3. H | 3968 | 9. $\zeta$ | 3730 |
| 4. $a$ | 3887·5 | 10. $\eta$ | 3717·5 |
| 5. $\beta$ | 3834 | 11. $\theta$ | 3707·5 |
| 6. $\gamma$ | 3795 | 10. $\iota$ | 3699 |

In all these stars the line K is either absent or very thin as compared with its appearance in the solar spectrum.* In the spectrum of Arcturus, which belongs to the solar type, this line exceeds in breadth and intensity its condition in the solar spectrum. The white stars may, therefore, be arranged in a series in which the line K passes through different stages of thickness, at the same time that the typical lines become narrower and more defined, and other finer lines present themselves in increasing numbers. Arcturus seems to present a spectrum on the other side of that of the sun in the order of changes from the white-star group.

The spectra of the planets were taken on the plan suggested by the author in 1864, in which the planet's spectrum is observed or photographed together with a daylight spectrum. These photographs show no sensible planetary modification of the violet and ultra-violet parts of the spectrum of the planets Venus, Mars, and Jupiter.

Numerous spectra of small areas of the lunar surface have been taken under different conditions of illumination, and during eclipses of that body. The results are wholly negative as to any absorptive action of a lunar atmosphere.

The author is preparing to attempt to obtain by photography any lines which may exist in the violet and ultra-violet spectra of the gaseous nebulæ. He also points out the suitability of the photographic method of stellar spectroscopy, first inaugurated by his researches, to some other investigations, such as—differences which may present themselves in the photographic region in the case of the variable stars, the difference of relative motion of two stars in the line of sight, the sun's rotation from photographic spectra of opposite limbs, and the spectra of the different parts of a sun-spot.

In the hope of throwing light on many physical questions suggested by the stellar photographs, the author has taken for comparison a number of terrestrial spectra, especially of hydrogen and calcium, under different physical conditions. As he is still pursuing this inquiry, he reserves an account of this part of his work.

* Messrs. Dewar and Liveing have found in their experiments similar relative changes of intensity of the lines of calcium corresponding to H and K in the *emission* spectrum of calcium.

## Art. XLIX.—*The Uranometria Argentina.*

It is now nearly ten years since Dr. Gould arrived in Buenos Ayres on his way to Cordoba, under appointment by President Sarmiento to establish there a national observatory for the Argentine Confederation. It was more than two years after his arrival before he was able to mount his meridian circle and begin regular observations with it. This interval was not allowed to run to waste, and Dr. Gould began a series of observations with the purpose of doing for the southern stars what Argelander had done so well thirty years before, in the *Uranometria Nova*, for the stars of the northern sky.

Th results of this undertaking are now published in a quarto volume of 385 pages, and an atlas of 14 large maps of stars. The Uranometria Argentina will probably always be the standard of reference for the southern stars visible to the naked eye. Any information about it will therefore be acceptable to the readers of the Journal.

The quarto volume is printed in parallel columns of Spanish and English. It forms the first volume of the *Resultados del Observatorio Nacional Argentino.* It is beautifully printed by Coni at Buenos Ayres.

It consists of eight chapters. The first contains the history of the work. In the second, on *Standards of Magnitude*, Dr. Gould explains the several steps in forming standards for a symmetric and continuous series of gradations of brilliancy expressed in tenths of magnitudes, such that the round units themselves should coincide as nearly as possible with those of Argelander in his *Uranometria Nova.* A zone of stars 10° in breadth was selected, whose altitude was the same at Bonn and at Cordoba, in which 722 stars were selected as types. This series of standards was, in the end, minutely compared with the corresponding determinations of Argelander, and those of Heis in his *Atlas Coelestis Novus*, also with the magnitudes given in the *Durchmusterung* and in the zones of Lalande and Bessel. Some of the stars had been observed at Albany by Dr. Gould in 1858. A table of these type stars is given with their magnitudes as stated by these several authorities. For stars brighter than the sixth magnitude, the adopted scale ranges lower by an insignificant amount than that of the Uranometria Nova. Dr. Gould's comparisons of scales of magnitude in this chapter and in the fourth chapter are entirely independent of the like elaborate comparison by Professor C. S. Peirce in the ninth volume of the Annals of the Harvard Observatory.

The third chapter is devoted to the constellations and their

nomenclature. It opens with a history of what was done by Bayer, who first broke away from the traditional catalogue and figures of Ptolemy, by Lacaille, Bode, Sir John Herschel, Baily and others in their attempts to arrange the stars of the southern sky in constellations, and give the stars distinctive names in them. The principles which Dr. Gould has finally acted upon are expressed in six rules.

The first of these is that the constellations of Ptolemy and Hevelius, together with those adopted or introduced by Lacaille, are to be retained, and no others. *Argo* disappears *nominally*, being replaced by *Carina*, *Puppis* and *Vela*.

The other rules refer principally to the names of the stars and the constellations, except the third, which is perhaps the most important.

"The boundaries must be so arranged that the constellations shall include all stars denoted by Greek letters which were assigned to them by their authors (unless such arrangement has been superseded by later accepted authority), together with all others as bright as the sixth magnitude, which are referred to them by general usage. The boundary lines are to be formed, wherever possible, by meridians of right-ascension and parallels of declination for the mean equinox of 1875·0. When this is not feasible they should consist of regular curves as near as may be to great circles, and their positions be defined by points of intersection with meridians and parallels."

Dr. Gould has described boundaries for the several constellations up to the parallel of 10° of N. Declination, where the catalogue terminates. In approaching this limit he says that he has kept in mind the possibility of some future attempt to establish an analogous system in the northern hemisphere, and adds that this is clearly not to be thought of at present, but may at some not distant day be regarded as desirable.

The notation adopted for the stars in each constellation is given at length in the carefully prepared notes to the several constellations.

This fixing of the boundaries of the constellations, in the southern sky, is a subject of great importance. Would it not be well for the *Astronomische Gesellschaft*, or some other authority in astronomy, to consider Dr. Gould's boundaries, and, if they are approved, urge their acceptance by all astronomers?

The fourth chapter explains the determination of the magnitudes of the stars from those of the type belt, and gives a comparison of the magnitudes adopted with the magnitudes of Argelander, Heis, Lacaille, Behrmann, Lalande, Bessel and Herschel.

The fifth chapter contains the catalogue of the stars arranged by constellations. Only those stars are included that are

brighter than 7·1 magnitude, and are within 100° of the south pole, in all 7730 in number. The stars bear current numbers, in order of R. A., and the synonym in other catalogues, and the magnitude, together with the R. A. and declination are given for each star. Of the number mentioned, 6733 belong to the southern heavens, and 997 to the belt of 10° in width, north of the equator. They belong to 66 different constellations.

About 100 pages are occupied with the sixth chapter, which contains notes to the catalogue. Most of these are devoted to the magnitudes of stars, with special reference to their being possibly variable. This property of variability Dr. Gould believes to be much more frequent than has been hitherto supposed.

Chapter VII describes the atlas, which "consists of thirteen special charts, together with a fourteenth or general one, which presents at a single view the whole region included in our work, and serves as an index map both for the others and for the constellations."

These charts are drawn upon the stereographic projection, and correspond to a sphere having a radius of one meter. The stars are represented by circular black dots, having their areas proportional to their respective amounts of light; and upon the map there is given a scale to half magnitudes, but the actual drawing shows the separate stars to a nearer approximation.

"The names and distinguishing letters of the stars have been omitted, and the names of the constellations been placed in a compact form in those positions least likely to distract the attention of the observer" in order to have the real aspect of the sky as nearly reproduced as possible.

The practical inconvenience entailed by this condition of not having the stars' letters given in the first thirteen charts, is lessened by having the principal ones lettered in Chart XIV.

The charts themselves are sumptuously lithographed on heavy paper and in excellent style throughout. Perhaps nowhere is the artistic skill of the lithographer, Mr. Julius Bien, better shown than in the beautiful representation he has given of the Milky Way. Dr. Gould, after referring to the labor expended on the mapping of this part, goes on to say, "To astronomers dwelling near the level of the sea, or in the neighborhood of large cities, or where, for any other reason, the meteorological conditions are not especially favorable to transparency in the atmosphere, the brilliancy of the Milky Way as here depicted may seem excessive. But this is not so in any of those impressions which I have personally examined; none of them exaggerating in general its brightness as seen at Cordoba under favorable circumstances."

Chapter VIII, which is a dissertation on the distribution of the stars (occupying 35 pages), opens with a table showing "the number of stars, of each grade of brilliancy from $7^m{\cdot}0$ upward, which are to be found south of the parallel of ten degrees north declination." The results of this table are:—

| Magnitude. | No. Stars South Declination. | No. Stars North Declination. |
|---|---|---|
| 0·0 to 2·0 | 19 | 3 |
| 2·1 to 3·0 | 66 | 6 |
| 3·1 to 4·0 | 166 | 29 |
| 4·1 to 5·0 | 321 | 55 |
| 5·1 to 6·0 | 1238 | 174 |
| 6·1 to 7·0 | 4884 | 724 |

Putting $\Sigma_m$ to represent the total number of stars contained in the catalogue to the $m$th magnitude inclusive, he finds

$$\Sigma_m = 0{\cdot}54896\ (3{\cdot}9111)^m$$

to be a near approximation to the number of stars of each magnitude contained within the limits. Then follows a careful and somewhat elaborate comparison between the numbers of stars assigned to the several magnitudes in the Durchmusterung, the Uranometria Nova and the Atlas Coelestis of Heis. Dr. Gould sums up the result of his extremely interesting investigation (p. 368) as follows:

"1. There is in the sky a girdle of bright stars, the medial line of which differs but little from a circle, inclined to the galactic circle by a little less than 20°.

2. The grouping of the fixed stars brighter than $4^m{\cdot}1$ is more systematic, relatively to that medial line, than to the galactic circle; and the abundance of bright stars in any region of the sky is greater as its distance therefrom is less.

3. The known tendency to aggregation of faint stars toward the Milky Way is according to a ratio which increases rapidly as their magnitudes decrease, and the law of which is such that the corresponding aggregation would be scarcely, if at all, perceptible for the bright stars.

4. These facts, together with others which have been stated, indicate the existence of a small cluster, within which our system is eccentrically situated, but which is itself not far from the middle plane of the galaxy. This cluster appears to be of a flattened shape, somewhat bifid, and to consist of somewhat more than 400 stars, of magnitudes from the first to the seventh, their average magnitude being about 3·6 or 3·7.

5. The general distribution of the fixed stars according to magnitude does not appear capable of being well represented by any simple algebraic expression. Yet by adopting the data of the preceding paragraph, and supposing the several magnitudes of the stars in the cluster to follow the law of Probabilities, we obtain for each class of magnitudes a number, which

being subtracted from the observed number in the sky, leaves a system of distribution which may be represented by the expression $\Sigma_m = ab^m$ within the limits of the errors of observation.

6. The accordance thus obtained holds good for the stars of both hemispheres down to the lowest limits of magnitude for which trustworthy enumerations exist; and this whether we employ the numbers of the Durchmusterung, of Argelander's and Heis's Uranometries, or of this present work.

7. The form of the expression $\Sigma_m = ab^m$ is that which corresponds to the hypothesis that in general the stars are distributed at approximately equal distances from one another, and are of approximately equal intrinsic brilliancy. It is, however, not requisite for its applicability that their distribution be equable in all directions, but only that their number be proportional to the volume of the spherical shell within which they are contained.

8. Each of the authorites, and each hemisphere, affords data from which results essentially the same value for the ratio $b$, the difference in the data being in every case, represented by differences in the coëfficient $a$. The value thus obtained for $b$, corresponds to the light ratio 0·4028 for descending, or 2·4827 for ascending, magnitudes."

Then follows a description of the parts of the Milky Way, with its rifts and ramifications, and the gradations and contrasts of light. With this is a careful determination of the medial points and the breadth of the stream, the position of the galactic circle, and the numbers of stars on the two sides of the circle.

"Inferences of some cosmological importance are deducible from the tables just given. It cannot escape notice that the part of the Milky Way which lies between 160° and 225° of galactic longitude, or from $6^h$ to $8^h$ of right-ascension, is much the broadest of all; this corresponding to the region of widest separation of the branch-circles in the undivided portion of the stream. Moreover the narrowest parts are from $3^h$ to $5\frac{1}{2}^h$ and from $10\frac{1}{2}^h$ to $12^h$ of right-ascension, or, roughly, in the galactic longitudes 105° to 150° and 255° to 270°. These regions, which are also of preëminent brilliancy, correspond approximately to the place where the circles of the branches intersect each other; in short, there are sundry indications that the whole phenomenon of the Milky Way may become simplified by treating it as the resultant of two or more superposed galaxies."

H. A. NEWTON.

ART. L.—*On the Ivanpah, California, Meteoric Iron;* by CHAS. UPHAM SHEPARD, Emeritus Professor of Natural History in Amherst College.

FOR my knowledge of the discovery of this meteorite, I am indebted to Mr. C. C. Parry, of the Academy of Science of Davenport, Iowa, and to Mr. W. G. Wright, Naturalist at San Bernardino, California, from each of whom I received a few weeks ago, communications upon the subject, accompanied by small fragments from the mass, for my examination and analysis.

Before proceeding to the description of these, I may state the circumstances connected with this interesting discovery. The locality is situated in a region known as the Colorado Basin, within eight miles of Ivanpah, which place is about two hundred miles northeast of San Bernardino in Southern California.

The mass was discovered very recently by Mr. Stephen Goddard, who in returning one evening to his camp after a prospecting excursion, as he was crossing what is there called a *wash*, had his attention arrested by a singular looking bowlder. On striking it with his pick, he was still more surprised at the ringing sound produced by the blow. These observations led him to return the day following with a wagon, and to remove it to Ivanpah. From thence it was taken by Mr. Heber Huntington to San Bernardino, where it was placed for some time on exhibition at the store of Mr. Craig. From thence again, it has lately been transported to San Francisco, and deposited with Mr. Henry G. Hanks, the State Geologist; and will, in all probability, be preserved in the future geological collection of California.

### *Description of the Meteorite.*

It is oval in shape, having one of its sides somewhat flattened. Its surface is entirely covered with depressions or dents, "as if it had been patted all over with pebbles" or clam shells, while yet soft or plastic. The size and shape of these concavities are various, from one to four inches across; and in addition, there are three round holes an inch deep as if made by the little finger.* The weight of the mass is supposed to be one hundred and twenty pounds. Its dimensions are fourteen inches in length, by nine in breadth and seven in thickness.

The fragments in my possession (the largest weighing five grams) show a highly crystalline and homogeneous iron, requir-

* From this account, it would appear that the mass resembles the Orange River (South Africa) iron of 170 pounds weight in the Amherst College cabinet, which singularly enough has one of the finger-holes above noticed, which though not so deep, is as perfectly turned at sides and bottom, as if artificially formed.

ing no aid of etching to reveal the Widman figures; and prove, that it must belong to the order Megagrammic of my class of Siderites. Indeed it seems highly probable, that the crystalline structure of the entire mass is in conformity with that of a single individual. The cleavages, as most usual in these bodies, are octahedral; and reveal rather a coarse lamination. The schreibersite separating these thick laminæ (as brought into view by polishing and etching) is very thin; and runs in perfectly straight lines, dividing the polished surfaces off into rather broad, triangular and oblique angled spaces, whose areas again, are beautifully covered by very small irregular dots and characters, themselves distributed in parallel rows, but among which, continuous straight lines appear to be wanting,—the boundaries of the larger, triangular and quadrangular spaces only, consisting of rectilinear lines. There would therefore seem to be two varieties of schreibersite present; one in flat leaves, the other in wavy, semi-cylinders or irregular prisms. The latter may be the rhabdite of Reichenbach. Both kinds, however, are equally taken into solution by long digestion in aqua regia. Specific gravity = 7·65.

*Composition.*

| | |
|---|---|
| Iron | 94·98 |
| Nickel | 4·52 |
| Phosphorus | 0·07 |
| Graphite | 0·10 |
| | 99·67 |

No sulphur was present. For want of material, no examination was made for the metals, often detected in small quantities in meteoric irons.

Charleston, S. C., March 12, 1880.

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ART. LI.—*The Atomic Weight of Antimony: Preliminary Notice of Additional Experiments;* by JOSIAH P. COOKE.

[From the Proceedings of the American Acad. of Arts and Sci., Mar. 10, 1880.]

IN our previous paper on this subject,* we gave our reasons for the opinion, since fully confirmed, that the bromide of antimony is the most suitable compound of this element, as yet known, for determining its atomic weight; and the results of fifteen analyses of five different preparations of the bromide were published, which gave for the atomic weight in question the mean value 120·00 with an extreme variation between 119·4 and 120·4 for all the fifteen analyses, and between 119·6

* This Journal, III, xv, 41, 107, 1878.

and 120·3 for the six determinations in which we placed most confidence. The antimonious bromide used in these determinations was purified first by fractional distillation, and secondly by crystallization from a solution in sulphide of carbon. In the crystallized product thus obtained, the bromine was determined gravimetrically as bromide of silver in the usual way. Although it seemed at the time that the results were as accordant as the analytical process would yield under the unfavorable conditions, which the presence of a large amount of tartaric acid in the solution of the bromide of antimony necessarily involved; yet it was obvious that the agreement was far from that which was desirable in the determination of an atomic weight, and our chief confidence in the accuracy of the mean value—independently of its remarkable agreement with previous results—was based on the fact that the known sources of error tended to balance each other. Hence our conclusions were stated with great caution, and the hope was expressed that after a more thorough investigation of the subject we might be able "to return to the problem with such definite knowledge of the relations involved as will enable us to obtain at once more sharp and decisive results than are now possible." Unfortunately this investigation has been delayed by causes beyond our control.

In our previous paper, we described a simple apparatus which we devised for subliming iodide of antimony; and in a note to the paper we stated that we were applying the same process to the preparation of the bromide of antimony, and that it promised excellent results. Our expectations in this respect have been fully realized, and the product leaves nothing to be desired either as regards the beauty or the constancy of the preparation. The fine acicular crystals are perfectly colorless, and have a most brilliant silky luster. With ordinary precautions they can be kept indefinitely without change, and it is easy therefore to determine the weight of the material analyzed to the tenth of a milligram.

We have carefully studied the causes of error involved in the analytical process of determining bromine in an aqueous solution of bromide of antimony and tartaric acid by the usual gravimetric method. These causes we propose to discuss in a future more extended paper. In this preliminary notice, we have only space to state that we have satisfied ourselves that the small differences between the results previously obtained arose wholly from the analytical process, and not from any want of constancy in the material analyzed; and further that these sources of error are to a very great extent under control. Moreover, we have found that the volumetric determination of bromine by silver was not materially affected, if at all, by the

same causes. We have thus been led to devise a mode of testing the atomic weight of antimony, which, while it has all the advantages of the gravimetric method previously employed, is free from its sources of error.

If the atomic weight of antimony were 122·00, it would require 1·7900 grams of pure silver to precipitate the bromine from a solution of 2·0000 grams of antimony bromide, while if the atomic weight of antimony were 120·00 it would require 1·8000 grams of silver. Now it is easy to estimate volumetrically $\frac{1}{100}$ of this difference with great certainty. We therefore prepared with great care a button of *pure* metallic silver, which we annealed and rolled out to a thin ribbon. We then weighed out from two to four grams of bromide of antimony, prepared by sublimation as described above, and dissolved this salt in an aqueous solution of tartaric acid, which we then transferred to a liter flask and diluted to about 500 cubic centimeters. We next very accurately weighed out a quantity of silver slightly less than that which calculation showed was required for complete precipitation. This silver was dissolved in nitric acid, and the solution having been evaporated to dryness over a water bath, the silver salt was washed into the flask containing the bromide of antimony. As soon as the supernatant liquid had cleared, the small additional amount of a normal silver solution required to produce complete precipitation was run in from a burette, and measured with the usual precautions. We used no extraneous indicator, because it was important not to introduce any possibly new disturbing element into the experiment, and in the titration of bromine with silver the normal and familiar phenomena, which mark the close of the process, furnish a very sharp indication. The details of one of the determinations were as follows:—

The weight of the bromide of antimony used amounted to 2·5032 grams. To precipitate the bromine from the solution of this material 2·2404 grams of silver would be required if Sb = 122·00 and 2·2529 if Sb = 120·00. We weighed out, with as much accuracy as if we were adjusting a weight, the smaller of these two quantities of metallic silver, and after dissolving the pure metal in pure nitric acid, evaporating the solution to dryness and redissolving in water, we added at once the whole of this silver solution to the liter flask containing the solution of bromide of antimony, in the manner described above. It was then found that $12\frac{4}{10}$ cubic centimeters of a normal silver solution (one gram of silver to the liter) were required to complete the precipitation. It will be seen that the weights of the bromide of antimony and silver used could be thus determined with the most absolute precision, and we have the

greatest confidence in these values to the $\frac{1}{10}$ of a milligram. Moreover, it will be noticed that the volumetric method is only used to estimate the difference in the atomic weight which has been in question, and that if the method were only accurate to the $\frac{1}{10}$ of the quantity to be measured it would give us the value of the atomic weight within $\frac{2}{10}$ of a unit; while if, as we had reason to believe, the process was accurate within one per cent, it would fix the atomic weight within $\frac{2}{100}$ of a unit.

By the method just described, the following results were obtained: The letters *a* and *b* indicate different preparations.

| Wt. of $SbBr_3$ taken. | Total wt. of Ag used. | Per cent of Br Ag=108 Br=80. | Corresponding value of Sb. |
|---|---|---|---|
| *a* 1. 2·5032 | 2·2528 | 66·6643 | 120·01 |
| *a* 2. 2·0567 | 1·8509 | 66·6620 | 120·02 |
| *a* 3. 2·6512 | 2·3860 | 66·6644 | 120·01 |
| *b* 4. 3·3053 | 2·9749 | 66·6696 | 119·98 |
| *b* 5. 2·7495 | 2·4745 | 66·6653 | 120·01 |
| Mean value, | | 66·6651 | 120·01 |
| Mean value of fifteen gravimetric determinations previously published, | | 66·6665 | |
| Theory Sb. 120 requires | | 66·6666 | |
| " Sb. 122 " | | 66·2983 | |

In order still further to control the work, we collected the bromide of silver formed in the last two determinations, washing the precipitate with the precautions which experience had shown to be necessary, and determining its weight, first, after drying at 150° C., and, secondly, after heating to incipient fusion. In *b* 6 there was a loss of $\frac{1}{10}$ of a milligram; in *b* 7 a loss of $\frac{2}{10}$ of a milligram only at the second weighing. This is an absolute proof that there could be no sensible occlusion of any tartaric acid or any tartrate by these precipitates, and, as stated in our original paper, the same test was frequently applied, although not always, in our previous determinations. It is also evident that these last experiments give us two essentially distinct determinations of the atomic weight, although the materials employed were identical with those of *b* 4 and *b* 5.

| Wt. of $SbBr_3$ taken. | Wt. of Ag Br determined. | Per cent of Br Ag=108 Br=80. | Corresponding value of Sb. |
|---|---|---|---|
| *b* 6. 3·3053 | 5·1782 | 66·665 | 120·01 |
| *b* 7. 2·7495 | 4·3076 | 66·667 | 120·00 |
| Mean value, | | 66·666 | 120·00 |

Lastly, it is obvious that these gravimetric determinations, taken in connection with the corresponding volumetric results, give us the most conclusive evidence of the purity, both of the

metallic silver used, and also of the bromide of antimony, which is the basis of this atomic weight investigation. By comparing *b* 6 and *b* 7 with *b* 4 and *b* 5 respectively, we obtain the following data:—

1. 2·9749 gram of silver gave 5·1782 gram bromide of silver.
2. 2·4745 " " " 4·3076 " " "

Hence it follows that, as shown by these experiments, the proportions of the silver to the bromine were respectively:—

| | | | | | |
|---|---|---|---|---|---|
| 1. | 108·00 | Silver to | | 79·99 | Bromine. |
| 2. | 108·00 | " | " | 80·01 | " |
| Mean value, | 108·00 | " | " | 80·00 | " |

This is the ratio of the atomic weight of silver to that of bromine, and corresponds to the second decimal place with the determinations of Stas as well as with those of Dumas.

In conclusion it gives us pleasure to express our obligations to Mr. G. De N. Hough and Mr. G. M. Hyams, two students of this laboratory, who have greatly aided us in the experimental work of this investigation.

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ART. LII.—*Daubrée's Experimental Geology: Part II, Experimental Study of Meteorites with reference to certain Cosmical Phenomena;* noticed by J. LAWRENCE SMITH.

Études Synthétiques de Geologie Expérimentale; par A. Daubrée, Deuxiéme partie—application de la methode expérimentale a l'étude de divers phénomènes cosmologigues.

THE first part of Professor Daubrée's valuable work on experimental geology appeared several months since.* The second part, embracing about 350 pages, is exclusively devoted to an experimental study of the structure and genesis of meteoric minerals, and to the bearing of the facts on the constitution of the universe.

The first chapter of the part before us is devoted to the study of the phenomena attending the fall of Aerolites and to their classification. The statement is reaffirmed, "that the two months, August and November, remarkable for showers of shooting stars, have no particular prominence over other months as regards the number of falls of meteoric stones." It is important that this fact should be clearly stated and reiterated, since many scientists are disposed to connect these two classes of phenomena; until shooting stars and meteoric stones are treated of wholly independently, no approach will be made to a correct theory in regard to the origin of the latter.

* This Journal, August, 1879, p. 150.

The composition of meteorites is reviewed, and the well-established fact that but few minerals enter into the constitution of these bodies, however different they may be in physical characters.

The metallic iron, forming either the entire mass or disseminated in small particles through the stones, is invariably a nickeliferous iron, containing a little cobalt and a trace of copper. The stones commonly consist almost entirely of bronzite* or enstatite (sometimes with magnesia as the only protoxide element) and olivine in various proportions, with particles of iron in smaller or greater quantity disseminated through the mass. The next most abundant non-metallic mineral is anorthite, which is found in that class of meteorites called eukrite. Besides these, a few other minerals are more or less constant, but usually in small quantities and some of them in minute proportions, such as troilite, schreibersite, chromite, daubréelite, etc. Other well-known physical and chemical characters are referred to in the same chapter.

The most interesting part of Professor Daubrée's labors relates to the fusion of meteorites and their artificial imitation. In the first class of experiments the results show that the fusion of meteoric stones does not alter materially the character of the minerals contained in them, except that the crystals of the minerals become better characterized, as seen by examination under the microscope.

The fusion of the eukritic meteorites present some remarkable features; "they yield a product wholly different from the other magnesian meteorites, namely: a vitreous mass sometimes striped by a commencement of devitrification, but without crystals of olivine or enstatite." "In the same experiments, a substance is obtained that does not appear to have been seen in the magnesian meteorites; this is titanium (in the state of carbo-azotide) recognized by its characteristic color and by its not being attacked by acids.

The imitation of meteorites by the reduction of terrestrial rocks was successfully made by fusing rocks consisting principally of pyroxene (a mineral closely related to enstatite) and olivine. In operating upon olivine and lherzolite in large quantities, by fusing them in a crucible *brasqué*, buttons of iron were obtained giving regular figures when etched, and containing nickel and cobalt. In another series of experiments, hydrogen instead of carbon was used as the reducing agent on lherzolite and pyroxene, and the decomposition was effected at a low red heat. The oxide of iron was reduced to metallic iron, and the phosphates to phosphides, the products having a close

* The enstatite, also called bronzite, in its chemical character is a magnesian pyroxene.

chemical analogy to what is found in meteorites. A portion of this part of the volume contains a minute description of the San Catharina meteoric iron, which possesses much interest and is worthy of a still more extended study.

In the second chapter, the deep-seated rocks of our globe are compared with meteorites. There is much that is interesting in the statements in regard to olivine and the transformation of serpentine into olivine. In this connection, Professor Daubrée makes some interesting statements with reference to the association of platinum with serpentine. The presence of metallic iron in native platinum is a distinguishing characteristic and would lead us to infer some connection between the rock matrix of platinum and meteorites.

Daubrée's observations and experiments in regard to the cosmic bodies from which meteorites are derived is of considerable interest, embracing as they do the supposable conditions under which the minerals were formed. In regard to what he considers minerals of reduction—as the iron and schreibersite—he is inclined to attribute their reduction to an atmosphere of hygrogen acting on the rocks at a more or less elevated temperature; how high the temperature was it is impossible to decide. He says: "It was without doubt an elevated temperature, because the anhydrous silicates, as olivine and pyroxene, are the products; yet at the moment of solidification and crystallization, the temperature appears to have been inferior to that which I employed in my previous experiments [those of fusing the meteorites]. Two facts would lead us to this conclusion." "The elevated temperature, produced in the laboratory, resulted in the formation of distinct and large crystals, such as are never found in meteorites. It is worthy of note that the siliceous substances which compose ordinary meteorites are always in small confused crystals, notwithstanding the extreme tendency of the minerals to crystallize. Beside this production of small confused crystals, there is a manifest tendency for them to assume a globular form."

The author compares meteorites with the rocks of our globe and brings out the contrast in a striking manner, showing their entire dissimilarity with our sedimentary rocks; and also with granite, gneiss, mica schist and other rocks of this family, whose minerals, as tourmaline and other silicates, are never present in meteorites. It is only below the granites that we encounter rocks at all analogous to meteorites; and in these occur all the elements and many of the minerals that are to be found in them. This, in connection with what the spectroscope has developed, points to a unity in the origin of celestial bodies and in the constitution of the universe, a fact illustrated in various lights by the author.

He observes: "Whatever their origin and orbits, the meteorites that fall on our planet show us one of the great methods of change carried on in the universe, consisting in the distribution of the fragments, derived from the destruction of certain stars, planets or asteroids, among other suns and planets. Such occurrences are not accidental or exceptional phenomena, but facts under a law of the universe. In establishing a close relation between meteorites and the deep-seated rocks of our globe, we not only unravel remote phases in the history of our own globe, but establish the intimate relation that exists between the different parts of the universe." "It is thus that geology, taken in its broadest sense, has an intimate relation to physical astronomy, for if it receives light, it also contributes light."

The second section of this part of Professor Daubrée's work treats of the mechanical phenomena connected with meteorites. Under this head are included the globular structure which characterizes many of the minerals, the polyhedral forms of meteorites, and the pitting on their surface. This last characteristic is treated at great length, and is attributed to sudden heating in connection with great gaseous pressure. Numerous examples are given of experiments on stone, iron and steel with gunpowder and dynamite; and over forty pages of the work are devoted to the details connected with them.

In the application of this part of the subject, Professor Daubrée says: "Those meteorites, which have a fragmentary structure and are very often polyhedral in form, exhibit on their surface the effects of the action of compressed and heated gases, the indentations affording evidence of a wearing action by air in a cyclonic movement." When meteoric masses have been isolated from each other after entering the atmosphere their surface, during their course, has been exposed to compressed and incandescent gases, from the time that they became luminous to that when they have exploded, at which time the incandescence ceases. The track of a meteorite while thus incandescent is often over one hundred and twenty-five miles, and takes several seconds. During this time, it certainly cannot preserve, in the midst of powerful erosive action, its polyhedral form, with well defined angles and edges." These deductions are certainly reasonable. It is evident that the powerfully compressed air is the cause of their frequent explosions attended with rupture of the mass; for the compression of the air may be supposed to reach 1000 atmospheres, and the bolides are turned and twisted in every direction in the midst of this condensed gas.

In the latter part of this work the other peculiar physical characters of meteorites are considered: the black veins, the

marbled structure of the coating of some of them, and also the pulverulent variety of meteorites, or, as it is sometimes called, meteoric dust, etc.

I have thus given a statement of some of the prominent features of this most admirable work on the mineral and geological study of meteorites, embracing not merely descriptions, but also the results of well directed experiments, and comprehensive philosophical conclusions. We have nothing like it in our scientific literature; and being the result of the labors of so distinguished a geologist as Professor Daubrée, one who has devoted much thought and labor to the subjects of which he treats, it deserves the closest study. The first part of the work I have not referred to, for it has been made known to us briefly in a former notice in this Journal, and quite fully in Professor Dana's recent edition of his Manual of Geology, which work cites many of the facts and conclusions, and some of its excellent illustrations.

In the preparation of both parts of this volume, the publisher has done his part most thoroughly, the paper and typography being such as are rarely equalled in scientific publications, and the illustrations in the text, which are very numerous, being of unusual beauty. The work is one that should find its way to the library of every geologist.

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ART. LIII.—*Bastnäsite and Tysonite from Colorado;* by O. D. ALLEN and W. J. COMSTOCK. (Contributions from the Laboratory of the Sheffield Scientific School. No. LX.)

THE material for the investigation, the results of which are here given, was received from Messrs. S. T. Tyson and H. E. Wood, to whom our thanks are due.

The first mineral examined was found by careful qualitative analysis to contain only the metals of the cerium group, fluorine and carbonic acid, with a trace of iron. Its characters are as follows: Hardness=4–4·5. Sp. gr.=5·18, 5·20. Luster vitreous to resinous. Color reddish brown. Streak light yellowish gray. Infusible. Is very slightly attacked by hydrochloric acid, without perceptible evolution of carbonic acid. Strong sulphuric acid dissolves it with evolution of carbonic and hydrofluoric acids. Strongly heated in a closed tube shows scarcely a trace of moisture. The direct results obtained by analysis are:

| | I. | II. | Mean. | | Swedish bastnäsite by Nordenskiöld. | |
|---|---|---|---|---|---|---|
| $Ce_2O_3$ | 40·88 | 41·21 | 41·04 | 75·80 | 28·49 | 74·26 |
| $(La, Di)_2O_3$ | 34·95 | 34·56 | 34·76 | | 45·77 | |
| $CO_2$ | 20·09 | 20·20 | 20·15 | | 19·50 | |

By converting a known weight of the mixed oxides of the mineral into anhydrous normal sulphates, the joint atomic weight of the metals was found to be 140·2. If from the carbonic acid obtained, an amount of the bases is calculated sufficient to form normal carbonate, the remainder of the bases calculated as metals and the fluorine estimated by difference, the mean becomes:

| | | Ratio. |
|---|---|---|
| $(Ce, La, Di)_2O_3$ | = 50·13 | ·153 |
| Ce, La, Di | = 21·82 | ·155 |
| $CO_2$ | = 20·15 | ·458 |
| Fl | = 7·90 | ·416 |
| | 100·00 | |

$$R_2O_3 : R : CO_2 : Fl = 1 : 1·01 : 3 : 2·72,$$

corresponding to the formula

$$R_2Fl_6 + 2\,R_3(CO_3)_3$$

in which R=Ce, La and Di. If the atomic weight of R=140·2, as found in the present case, the formula requires:—

| | |
|---|---|
| $(Ce, La, Di)_2O_3$ | = 49·94 |
| Ce, La, Di | = 21·32 |
| $CO_2$ | = 20·07 |
| Fl | = 8·67 |
| | 100·00 |

This mineral corresponds to that from Sweden described by Hisinger* under the name of Basiskfluorcerium. It was later re-investigated by A. E. Nordenskiöld,† who first ascertained its correct composition and called it *hamartite*. Huot had, however, previously called the mineral *bastnäsite*, after the locality. Nordenskiöld's analysis is given above for comparison.

Associated with *bastnäsite* occurs a mineral which proved to be an anhydrous normal fluoride of cerium, lanthanum and didymium, which we have examined with the following results:

H. = 4·5–5. Specific gravity = 6·14, 6·12.

Luster vitreous to resinous. Color pale wax yellow. Streak nearly white. B.B. blackens but does not fuse. In closed tube decrepitates, the color changes to a light pink, and shows slight traces of moisture. Insoluble in hydrochloric and nitric acids, but dissolves in concentrated sulphuric acid with evolution of hydrofluoric acid. Qualitative examination showed only the presence of fluorine and the metals of the cerium group.

Quantitative analysis gave the following results:

| | I. | II. | Mean. | Ratio. | |
|---|---|---|---|---|---|
| Ce | = 40·16 | 40·22 | 40·19 | ·284‡ | ·504 |
| La, Di | = 30·29 | 30·45 | 30·37 | ·220§ | |
| Fl (diff.) | = 29·55 | 29·33 | 29·44 | | 1·547 |
| | 100·00 | 100·00 | 100·00 | | |

* Œf. Ak. Stockh., 1838, p. 187. † Œf. Ak. Stockh., 1868, p. 399.

‡ Bürich's atomic weight (141·2) for cerium was used for calculations.

§ A known weight of the mixed oxides was converted into anhydrous normal sulphates, care being taken that the cerium should exist wholly as cerous sul-

From which is obtained the ratio

$$R : Fl = 1 : 3{\cdot}07.$$

The formula $(Ce, La, Di)_2Fl_6$ appears therefore to express the composition of the mineral. As this mineral differs essentially in chemical composition and physical properties from any mineral hitherto described, it should be regarded as a new species. We propose for it the name *Tysonite.*

*The process of analysis* used for both minerals was as follows: a solution was effected by strong sulphuric acid. After removing the excess of sulphuric acid the sulphates were dissolved in water. The bases were precipitated with ammonium oxalate, the oxalates ignited in air and finally in hydrogen in order to remove the slight amount of oxygen which $Di_2O_3$ takes upon ignition in air. The cerium in the mixed oxides was determined volumetrically by Bunsen's method. The $CO_2$ was determined by ignition in a combustion tube with lead chromate mixed with a little fused potassium di-chromate. A trial of this method with pure calcium carbonate mixed with calcium fluoride gave satisfactory results.

*Locality and mode of occurrence.*—The material first furnished to us by Messrs. Wood and Tyson came from a locality at that time unknown to them, and consisted of a few grams of fragments of crystals of bastnäsite, to some of which were attached portions of the tysonite, readily distinguishable by its lighter color and perceptible cleavage, which is wholly lacking in the bastnäsite. Mr. Tyson, having recently succeeded in reaching the locality, which is near Pike's Peak, has just placed in our hands for examination all the specimens which he could obtain, about a dozen crystals and fragments of crystals, the largest of which are upwards of an inch in diameter, mostly free but in some cases attached to feldspar.

The crystals are hexagonal in form, the only planes observed being O, I and *i*-2. On a single crystal can be seen the remains of pyramidal planes, but so rounded by abrasion that any measurements would be useless. The crystals are prismatic in habit, the smaller ones slender and somewhat elongated, the larger ones short and thick.

These specimens show an interesting relation between the fluoride and the fluo-carbonate. The smaller crystals consist wholly of fluo-carbonate; in the larger crystals, however, a portion occupying the interior, about equally distant from the basal planes, usually about half an inch from them and extending nearly to the lateral planes, consists of the *fluoride*. The thickness of this band varies with the length

phate. The weight of cerium in the oxides used being known (and its state of oxidation), the joint atomic weight of the lanthanum and didymium may be calculated from the mixed sulphates obtained. Two such experiments gave the numbers 137·9 and 138·1, the mean of which (138) was used for calculations.

of the crystals from a few lines to half an inch. The line of demarkation between it and the fluo-carbonate is quite distinct. This mode of occurrence of the two compounds, being such as is often seen in crystals which have undoubtedly undergone partial changes of composition, leads to the conclusion that the bastnäsite of Colorado was formed by a change of a fluoride into a fluo-carbonate. In the fluoride a distinct but not strongly marked cleavage exists parallel to the basal planes of the enclosing fluo-carbonate. In the latter we could detect no evidence of cleavage.

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ART. LIV.—*On Argento-antimonious Tartrate (Silver Emetic)*; by JOSIAH P. COOKE. (Contributions from the Chemical Laboratory of Harvard College).

As stated by us in our paper on the atomic weight of antimony,* this compound was originally obtained by Wallquist by precipitating nitrate of silver with tartar emetic, and was analyzed both by him and by Dumas and Piria. These chemists obtained respectively 27·31 and 28·05 per cent of oxide of silver. They appear however to have prepared the substance only in an amorphous form. As stated in the paper just cited, we first noticed the formation of crystals of the compound in a concentrated solution of antimonious chloride and tartaric acid, to which had been added an excess of argentic nitrate, and from the circumstances of their formation we were led to form a somewhat erroneous inference in regard to their relation to water. We find that the substance is far more soluble in this solvent than at first appeared. We have found from further investigation that one part of silver emetic dissolves completely in one hundred parts of boiling and in somewhat less than five hundred parts of water at 15° C. In one determination made by evaporation, a saturated solution, which had stood a long time at a temperature of 15°, we found that 1000 parts of water had dissolved 2·76 parts and in another 2·68 parts of the salt. There is obviously therefore no danger of the formation of this product in the precipitation of chlorine, bromine or iodine from solutions of the antimony compounds of these elements in tartaric acid, unless the excess of silver nitrate is larger and the solutions concentrated; and although we have most carefully looked for it in the precipitate we have never discovered it, except under the peculiar conditions described in our former paper, and our fear that it might be occluded by these precipitates was wholly unfounded.

It is evident from the above experiments that the solubility

* This Journal, p. 382.

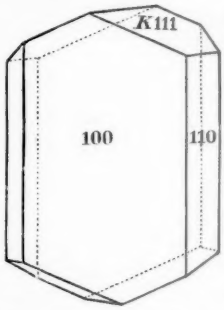
of silver emetic in water like that of cream of tartar and other salts of tartaric acid is very greatly increased by heat, and we were easily able to obtain good crystals of the compound in large quantities by dissolving the precipitate, obtained as Wallquist describes, in boiling water, and allowing the solution to cool. The crystals are colorless and have a very brilliant, almost an adamantine, luster.

From the reaction by which silver emetic is formed we should infer that the composition of the salt would be expressed by the symbol

$$Ag,\ SbO,\ H_2 \equiv O_4 \equiv (C_4H_2O_3)\ .\ H_2O.$$

This compound would theoretically contain 26·34 p. c. of silver, and, as a mean of three analyses, we obtained for the amount of silver in the crystals 26·30 per cent, as previously stated.

The crystals of silver emetic rapidly blacken in the light and are very easily decomposed by heat. This decomposition takes place at about 200° C. with a slight explosion. A very fine carbon dust is blown out of the crucible and a residue is left behind, which under the microscope is seen to consist of spangles of metallic silver mixed with an amorphous powder. Almost the whole of the powder dissolved easily in a solution of tartaric acid, and it evidently consisted of $Sb_2O_3$. In one experiment we weighed the silver emetic and the product, and found that 0·8460 gram. of the salt left 0·5304 gram. of residue. If the residue consisted solely of silver and $Sb_2O_3$, theory would require 0·5200 grams, and it can be seen from this how perfect the decomposition was. It is obvious therefore that were this compound occluded as we at first feared, it would have made itself evident on drying the precipitates.




Mr. W. H. Melville, assistant in this laboratory, has made the following crystallographic measurements of the crystals whose formation and reactions we have described.

*Angles between normals.*

$(111) \wedge (100)$ 70° 19½′

$(111) \wedge (\bar{1}11)$ 70° 17′

$a : b : c = 1 : 1{\cdot}386 : 0{\cdot}571$

| | | I. | Measured. |
|---|---|---|---|
| 100 | 110 | 54° 12′ | 54° 19′ |
| 111 | 110 | 54° 51′ | 54° 54′ |

The pinacoid planes were irregular and the angles can only be regarded as approximate.

System Trimetric with hemihedral habit.

Observed planes + $x$ | 111 | | 100 | | 110 | | 011 | ?

In the following table the crystallographic ratios are compared with those of the acid tartrates of rubidium, cæsium and potassium, formerly measured by us, and which have the same general form and hemihedral habit.

| | Vertical. | Macro. | Brachy. |
|---|---|---|---|
| Acid tartrate of cæsium | 0·661 | 1 | 0·694 |
| Acid tartrate of rubidium | 0·695 | 1 | 0·726 |
| Acid tartrate of potassium | 0·737 | 1 | 0·711 |
| Silver emetic | 0·412 | 1 | 0·721 |

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ART. LV.—*The Sternum in Dinosaurian Reptiles;* by Professor O. C. MARSH. (With plate XVIII).

THE presence of a sternum in Dinosaurs has long been in doubt, as hitherto this element has not been found in position, or identified with certainty among the known remains of the group. The evidence in favor of an ossified sternum in these reptiles rests mainly on a single bone, found, in the Jurassic of England, with the remains of *Ceteosaurus*, and described by Phillips.* Owen subsequently accepted this determination, and reproduced the original figure of this supposed sternum.† A few other specimens have been referred, with doubt, to the sternum of Dinosaurs, but apparently without any particular reason for the reference.

The Yale Museum has recently received a nearly complete skeleton of *Brontosaurus excelsus*, one of the largest known Dinosaurs. This huge skeleton lay nearly in the position in which the bones would naturally fall after death, and fortunately the entire scapular arch was in excellent preservation. The coracoids were in apposition with their respective scapulæ on each side, and between them lay *two* flat bones, that clearly belong to the sternum. This discovery, as interesting as it was unexpected, removes the main uncertainty about the scapular arch of Dinosaurs, and likewise indicates a new stage in the development of this structure, not before seen in adult animals.

These two sternal bones are suboval in outline, concave above, and convex below. They are parial, and in position nearly or quite joined each other on the median line. The anterior end of each bone is considerably thickened, and there is a distinct facet for union with the coracoid. The posterior end is thin, and irregular. These bones are shown in position

* Geology of Oxford, p. 268, 1871.

† Palæontographical Society, p. 31, 1875.

on Plate XVIII, figure 1, and one of them is more fully illustrated in figure 2. The inner anterior margin of each bone is smooth and rounded, and gives no evidence of union with an episternal element, which the vacancy there suggests. The amount of cartilage between these two sternal bones, or posterior to them, is not indicated by the present specimens. They were evidently separated by cartilage from the coracoids.

The nearest analogy among living forms to this double sternum may perhaps be found in immature birds. A close resemblance is apparent in the scapular arch of the young American Ostrich, represented on the same plate, figure 3. If the ossification of the sternum were permanently arrested at this stage, it would afford almost precisely the structure seen in the genus *Brontosaurus;* and this is evidently the true explanation of the fossil specimens here figured.

It is more than probable that, in many Dinosaurs, the sternum long remained cartilaginous, or so imperfectly solidified that it is not usually preserved. Several specimens of the genus *Camptonotus*, found nearly in their natural position, were apparently destitute of an ossified sternum. The large size, and doubtless great age, of the specimen of *Brontosaurus* above mentioned may perhaps have been the cause of its more perfectly developed sternum.

Yale College, New Haven, April 11, 1880.

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ART. LVI.—*On the Southern Comet of February*, 1880; by B. A. GOULD.

ON the evening of February 2nd, before the twilight was fully past, my attention was drawn to a remarkable streak of light in the southwest, which extended through about 18°, at an angle not much inclined to the vertical. Its lower extremity was perhaps 20° above the horizon, and the brightness was in no part much, if indeed any, greater than that of a star of the $5\frac{1}{2}$ magnitude. It seemed to taper in both directions, fading away at each extremity, and to be between 1° and 2° wide in the middle. A moment's reflection assured me that what I saw must be part of the tail of a comet, the lower portion being obscured by haze and its nucleus being below the horizon, which was concealed by a bank of clouds. No time was lost in preparing for an accurate drawing of its position, but the mist and clouds obscured it completely within a very few minutes, before any delineation could be made. Messrs. W. G. Davis and C. W. Stevens did, however, plot from memory upon the index-map of the Uranometry a sketch of its position and form, which seemed correct to both.

Inquiries the next morning showed that the same phenomenon had been observed on the evening of February 1st, by several persons, and one assured me that he had noticed it on the evening of Saturday, January 31st. All had supposed it to be connected in some way with the burning of grass or bushes, an occurrence which is here so frequent as to attract little attention, but which caused me useless labor and inquiry on more than one occasion during the first year of my residence in this country.

In the evening the ray or streak was about 30° long, and a little brighter than on the previous night, and it had moved laterally northward. Still, a careful search, beginning immediately after sunset, failed to discover the head, or indeed any increase of brightness in the vicinity of the horizon, although the direction of the tail seemed toward the position of the sun. Careful drawings were independently made, on this and each subsequent evening during its visibility, by Mrs. Gould and Mr. C. W. Stevens; the maps Nos. 2 and 3 of the Uranometry affording an excellent means for very minute delineation.

On the 4th I saw the head for a few moments in the twilight. It scarcely seemed brighter than Encke's comet appeared under similar circumstances at its last perihelion; but it was much larger and had a coarse and undefined aspect. No nucleus was visible. There was no opportunity to discover any comparison-star, before it was lost in the mists of the horizon; but a rough position was obtained by means of the setting-circles of the equatorial. This gave, for $5^h\ 27^m\ 55^s$ of Cordoba sidereal time, R.A. $22^h\ 24^m\ 10^s$, Decl. $-31°\ 29'{\cdot}1$. The altitude of the comet having been less than 2° 42′, no great reliance can be placed on this determination, which was moreover crude in other respects.

On February 5th, I obtained tolerably good comparisons with an undetermined star, the approximate position of which is $22^h\ 41^m\ 40^s$, $-32°\ 27'$ for the mean equinox of 1880·0; and from that date to February 19th, there were but two evenings on which observations were not secured, the sky having been especially propitious during that period. The tail, which I think was brightest February 6th or 7th, although then not more brilliant than the Milky Way in Taurus, maintained its inordinate length of from 35° to 40° until it faded from view, which took place only five days before the head became invisible in the $11\frac{1}{4}$ inch equatorial. Indeed it was with the greatest difficulty that I was able to observe it on the 19th, when it was only to be recognized as a slight whiteness in the field, unnoticeable without special attention. No nucleus was visible at any time during the whole duration of its visibility, nor was there any definite form or even perceptible outline to

the head, excepting on one or two nights at the beginning of the series of observations. It then exhibited an elongated form, somewhat rounded at its anterior margin, and shading away to form the tail, which was but little inferior in brightness, and seemed lost from view in the telescope in consequence of its lateral expansion almost as much as by any defect of its total light. On the 20th, the comet could not be detected in the telescope, although my ephemeris was so accurate as to leave no doubt concerning its position in the field.

The positions of the comparison-stars employed on the 7th, 8th and 11th cannot be sharply determined for several months. The observations on other days give the following results which are uncorrected for parallax, or aberration.

| Cordoba M. T. | R.A. | Decl. |
|---|---|---|
| | h m s | ° ′ ″ |
| 1880, Feb. 6, | 8 37 56·3 | 22 58 32·9 | −32 55 55·0 |
| 9, | 8 47 37·8 | 23 50 51·4 | 33 45 41·7 |
| 12, | 8 23 40·8 | 0 39 51·0 | 33 6 15·3 |
| 14, | 8 24 49·3 | 1 9 21·8 | 32 2 48·0 |
| 15, | 8 27 38·9 | 1 22 58·1 | 31 23 19·1 |
| 17, | 8 37 51·8 | 1 47 43 9 | 29 54 32·5 |
| 18, | 8 34 4·5 | 1 58 55·2 | 29 7 12·8 |
| 19, | 9 0 26·8 | 2 9 34·4 | −28 17 44·8 |

The excessive length of the narrow tail, its lack of gradation in brilliancy, and the relative faintness of the head, formed very notable characteristics. But, to my astonishment, on computing a parabola from the observations of February 6th, 9th and 12th, I found reproduced the orbit of the Great Comet of 1843. The almost incredibly small perihelion distance suggests in each case the origin of the huge tail; but the other elements were almost equally similar. A second orbit, from observations embracing the twelve days' interval from February 6th to 18th, proved equally similar to the orbit resulting from Hubbard's unsurpassed discussion of the Comet of 1843; and leaves no doubt whatever in my mind as to the identity of the two bodies, notwithstanding that an ellipse of 532 years was found to afford the best representation of the series of observations in 1843 as a whole. The elements now obtained are these: which are expressed in Washington mean time, and referred to the mean equinox of 1880·0.

| | |
|---|---|
| T | 1880, Jan. 27$^{d}$·40479 |
| $\Omega$ | 6° 10′ 29″·6 |
| $\omega$ | 86° 18′ 19″·0 |
| $i$ | 144° 39′ 38″·8 |
| log $q$ | 7·7393644 |

This result leads, however, to a yet more remarkable inference. At the time of apparition of the Comet of 1843, its identity with that of 1668 was very generally discussed and credited. Only the circumstance that Hubbard found the total

series of observations to be more satisfactorily represented by an ellipse of much longer period served to weaken this belief to any extent. Nevertheless Hubbard showed that the corresponding diminution of the major axis was compatible with a probable error of only $\pm 11''{\cdot}32$ for a single observation, in place of $\pm 8''{\cdot}44$ to which this value would be reduced by the adoption of his final elements. The similarity of the comet's appearance to that of 1702 also attracted attention. The orbit calculated for that comet by Struyck in Amsterdam bears no similarity to the well-determined one of the comet of 1668; but Cassini, who observed the former, believed the two to be identical. The same opinion was maintained by Cooper in 1843. The tail in 1702 was 40° long, and its path was chiefly in the southern hemisphere, both which facts favor the supposition of identity. Nevertheless, while a computation by Petersen, using for 1702 the orbit of the Comet of 1843, showed that the roughly given geocentric path might thus be somewhat roughly represented, Schumacher considered that the resultant places were compatible neither with the position of the tail March 2, 1702, as described by Maraldi, nor with the observation of the ship-captain Brouwer, cited by Struyck.

The interval between the perihelion-passages of 1668 and 1702 differs from 34 years by only a few days; that between those of 1843 and 1880 is but a month less than 37 years. If these three apparitions belong, as I am convinced, to one and the same comet, we have the singular phenomenon of a rapidly increasing period, for the interval from the perihelion of 1702 to that of 1843 is just 141 years, which gives $35\frac{1}{4}$ years for the average length of the four intermediate periods. The assumptions that the increase of the period has been systematic and that no other important perturbations have affected the times of perihelion-passage give for the successive returns to perihelion the following dates:—

| | | |
|---|---|---|
| 1702 Feb. 23. | 1771 June 6. | 1843 Feb. 27. |
| 1736 July 12. | 1806 Dec. 12. | 1880 Jan. 27. |

The second Comet of 1806 appears to have passed its perihelion on December 28th of that year. The latest determination of its orbit is that by Hensel in 1862, the resulting inclination being essentially the same as that of the present comet. His other elements, however, are completely discordant, the form of the orbit being hyperbolic and the perihelion-distance large. It remains to be seen whether the observations could be represented by an orbit of different form and dimensions. There are other recorded apparitions which seem likely to have been returns of the same comet, such as those of the years 1533, 1468, and perhaps 1264; but I have not here the means of forming any careful opinion.

It might perhaps be supposed that no appearance of so impressive an object during the last two or three centuries could have failed to be recorded; but it does not seem so to me in the case of a comet whose orbit lies almost exclusively to the south of the ecliptic, so that it would in all probability be noticed in our northern hemisphere only in the northern winter.

The wonderfully small, and apparently diminishing perihelion-distance affords an explanation of the increasing period; for, according to the present elements, this distance is but 0·00549, while the sun's own radius is 0·00466. It seems impossible that one side of the coma should not suffer actual friction against the body of the sun, to say nothing of its traversing the densest portion of his atmosphere through a full semi-circumference. The mechanical resistance thus interposed must have acted to diminish the perihelion-distance; and we find this accordingly to have been 0·005538 after the passage of 1843, and 0·005487 after that of 1880. Yet, while this resistance and the lateral friction must during their continuance be causing a diminution of the radius-vector, it would appear that they have not diminished, but, on the contrary, increased, the major axis. This is a delicate and in some respects a difficult question; and since I have at present neither the requisite time nor books of reference at my disposal, I will not here enter into any of its details, but will confine myself to the statement that so far as I am able to form an opinion, the observed facts do not appear to conflict with theory.

A most interesting question arises regarding the densest portion of the tail. There is no reason to doubt that this pointed southward before the perihelion, as it did afterward. Now the comet's center of gravity passed from one side to the other of the sun, describing an arc of 180° in true anomaly in about $2^h\ 8^m$; and indeed described 141° 42′ in a single hour. If the tail in general consisted of the same particles before as after the hour of perihelion, it must have been actually severed by the body of the sun, surrendering of course a considerable amount of its material.

In conclusion, I append the dimensions and position of the tail for each night of its visibility, given by means of the declinations of the intersection of its axis with the principal meridians upon the chart, and the width at these points of intersection. They are derived from the drawings of Mr. Stevens, and confirmed by the absolutely independent and accordant ones of Mrs. Gould. The positions of the head are as measured from these drawings, made upon map 2 of the Uranometria Argentina, without any reference to the ephemeris; but the ephemeris from the elements already given accompanies them.

The observed length of the tail depended, to a considerable degree, upon the clearness of the night.

*Ephemeris for Washington mean Noon and mean Equinox 1880·0.*

| 1880. | R. A. | Decl. S. | log Δ. | log *r*. |
|---|---|---|---|---|
| | h m s | ° ′ ″ | | |
| Feb. 2 | 21 47 33 | 28 57 27 | 9·868303 | 9·533186 |
| 4 | 22 19 9 | 31 10 55 | 9·847750 | 9·622916 |
| 6 | 22 52 59 | 32 45 2 | 9·835007 | 9·691392 |
| 8 | 23 27 52 | 33 36 1 | 9·829783 | 9·746770 |
| 10 | 0 2 18 | 33 43 58 | 9·831555 | 9·793264 |
| 12 | 0 35 0 | 33 13 55 | 9·839451 | 9·833330 |
| 14 | 1 4 59 | 32 14 12 | 9·852389 | 9·868533 |
| 16 | 1 31 48 | 30 54 13 | 9·869191 | 9·899922 |
| 18 | 1 55 27 | 29 22 31 | 9·888754 | 9·928246 |
| 20 | 2 16 6 | 27 45 47 | 9·910133 | 9·954049 |
| 22 | 2 34 8 | 26 8 41 | 9·932570 | 9·977744 |
| 24 | 2 49 53 | 24 34 13 | 9·955492 | 9·999648 |
| 26 | 3 3 42 | 23 4 7 | 9·978483 | 0·020014 |
| 28 | 3 15 56 | 21 39 14 | 0·001251 | 0·039043 |

*Positions as measured from the drawings.*

| Date 1880. | | Feb. 2. | Feb. 3. | Feb. 4. | Feb. 5. | Feb. 6. | Feb. 7. |
|---|---|---|---|---|---|---|---|
| Cordoba M. T. | | 8h20m | 8h20m | 8h35m | 8h30m | 8h35m | 8h35m |
| | Head R. A. | | | 22h24m | 22h40m | 22h59m | 23h17m |
| | S. Decl. | | | 31°20′ | 32° 0′ | 32°50′ | 33°25′ |
| | End R. A. | 23h36m | 0h40m | 1h16m | 1h50m | 2h20m | 2h32m |
| | S. Decl. | 57° 0′ | 60° 0′ | 57°40′ | 55°40′ | 52°40′ | 49°30′ |
| Length seen | | 18° | 30° | 40½° | 42½° | 41½° | 40½° |
| At 22h40m | S. Decl. | 43°45′ | 38°20′ | 35° 0′ | 32°45′ | | |
| | Width | 1°40′ | 1° 0′ | 0°30′ | 0°15′ | | |
| At 23h0m | S. Decl. | 39°30′ | 42°40′ | 38°45′ | 35°43′ | 33°20′ | |
| | Width | 1°45′ | 1°40′ | 1°20′ | 0°45′ | 0°15′ | |
| At 23h20m | S. Decl. | 54°10′ | 46°50′ | 41°48′ | 38°45′ | 36°40′ | 34°20′ |
| | Width | 1°40′ | 1°40′ | 1°40′ | 1°38′ | 0°50′ | 0°15′ |
| At 23h40m | S. Decl. | | 50°50′ | 45°50′ | 41°45′ | 39°17′ | 36°25′ |
| | Width | | 1°40′ | 2° 0′ | 1°42′ | 1°35′ | 0°50′ |
| At 0h0m | S. Decl. | | 54°25′ | 49°10′ | 44°35′ | 41°15′ | 38°50′ |
| | Width | | 1°40′ | 2° 0′ | 1°50′ | 1°40′ | 1°35′ |
| At 0h20m | S. Decl. | | 56°55′ | 51°55′ | 46°55′ | 43°35′ | 40°52′ |
| | Width | | 2° 0′ | 2° 0′ | 2° 0′ | 1°50′ | 1°40′ |
| At 0h40m | S. Decl. | | 60° 0′ | 54°20′ | 49°30′ | 45° 3′ | 42°30′ |
| | Width | | 2° 0′ | 2° 0′ | 2° 0′ | 2° 0′ | 1°50′ |
| At 1h0m | S. Decl. | | | 56°35′ | 51°35′ | 46°55′ | 44°10′ |
| | Width | | | 2° 0′ | 2° 5′ | 2° 0′ | 1°50′ |
| At 1h20m | S. Decl. | | | | 53°20′ | 48°45′ | 45°38′ |
| | Width | | | | 2°10′ | 2°10′ | 1°50′ |
| At 1h40m | S. Decl. | | | | 55° 0′ | 50° 3′ | 46°55′ |
| | Width | | | | 2°10′ | 2°10′ | 2° 0′ |
| At 2h0m | S. Decl. | | | | | 51°50′ | 48° 5′ |
| | Width | | | | | 2°10′ | 2° 0′ |
| At 2h20m | S. Decl. | | | | | 52°40′ | 49°10′ |
| | Width | | | | | 2° 0′ | 1°50′ |

| Date 1880. | | Feb. 8. | Feb. 9. | Feb. 11. | Feb. 12. | Feb. 14. |
|---|---|---|---|---|---|---|
| Cordoba M. T. | | $8^h40^m$ | $8^h50^m$ | $8^h25^m$ | $8^h35^m$ | $8^h50^m$ |
| Head R. A. | | $23^h32^m$ | $23^h43^m$ | $0^h23^m$ | $0^h40^m$ | $1^h\ 7^m$ |
| | S. Decl. | 33°41′ | 33°50′ | 33°45′ | 33° 5′ | 32° 6′ |
| End R. A. | | $2^h45^m$ | $2^h53^m$ | $3^h12^m$ | $3^h32^m$ | $3^h50^m$ |
| | S. Decl. | 47°25′ | 45° 0′ | 40°50′ | 38°35′ | 36° 0′ |
| Length seen | | 39½° | 39½° | 35° | 34½° | 34° |
| At $23^h40^m$ | S. Decl. | 34°40′ | | | | |
| | Width | 0°15′ | | | | |
| At $0^h0^m$ | S. Decl. | 36°50′ | 35° 2′ | | | |
| | Width | 0°40′ | 0°15′ | | | |
| At $0^h20^m$ | S. Decl. | 38°45′ | 36°42′ | | | |
| | Width | 0°45′ | 0°25′ | | | |
| At $0^h40^m$ | S. Decl. | 40° 3′ | 38° 0′ | 35° 2′ | 33° 5′ | |
| | Width | 0°50′ | 0°40′ | 0°12′ | 0° 0′ | |
| At $1^h0^m$ | S. Decl. | 42° 0′ | 39°40′ | 36°55′ | 34°25′ | |
| | Width | 1° 5′ | 0°50′ | 0°10′ | 0°10′ | |
| At $1^h20^m$ | S. Decl. | 43°20′ | 40°30′ | 37°40′ | 35°25′ | 32°35′ |
| | Width | 1°30′ | 1°10′ | 0°20′ | 0°15′ | 0° 5′ |
| At $1^h40^m$ | S. Decl. | 44°35′ | 41°35′ | 38°35′ | 36°20′ | 33°20′ |
| | Width | 1°50′ | 1°20′ | 0°35′ | 0°13′ | 0° 5′ |
| At $2^h0^m$ | S. Decl. | 45°25′ | 42°30′ | 39°15′ | 37°25′ | 34°10′ |
| | Width | 1°50′ | 1°40′ | 0°40′ | 0°20′ | 0°10′ |
| At $2^h20^m$ | S. Decl. | 46°10′ | 43°50′ | 40°10′ | 37°50′ | 34°35′ |
| | Width | 1°40′ | 1°40′ | 0°50′ | 0°15′ | 0°15′ |
| At $2^h40^m$ | S. Decl. | 47° 0′ | 44°15′ | 40°25′ | 38°10′ | 35° 0′ |
| | Width | 1°30′ | 1°25′ | 0°40′ | 0°15′ | 0°10′ |
| At $3^h0^m$ | S. Decl. | | | 40°20′ | 38°25′ | 35°20′ |
| | Width | | | 0°30′ | 0°10′ | 0° 5′ |
| At $3^h20^m$ | S. Decl. | | | | 38°45′ | 35°40′ |
| | Width | | | | 0°10′ | — |
| At $3^h40^m$ | S. Decl. | | | | | 36°10′ |
| | Width | | | | | — |

Cordoba, March 3, 1880.

## SCIENTIFIC INTELLIGENCE.

### I. Chemistry and Physics.

1. *On the Formation of Ozone by the Slow oxidation of Phosphorus.*—Some doubts having been expressed as to the actual production of ozone by the slow oxidation of phosphorus, McLeod has made a series of qualitative experiments to ascertain whether it is so produced or whether hydroxyl is the product. Oxygen ozonized by the spark in a Siemens tube was passed through a U tube containing solutions of sodium carbonate, of potassium dichromate and sulphuric acid, and of potassium permanganate. The ozone passed readily through these tubes even when surrounded by boiling water. Hydroxyl, on the other hand, is readily decomposed by sodium carbonate, especially at 100°, transforms chromic into perchromic acid and evolves

oxygen with permanganate. The air in which phosphorus was slowly oxidizing was then passed through the same U tube, and then into a solution of potassium iodide and starch. This solution became blue in all cases, when the U tube was cold as when it was heated to 100°. The addition of a second tube containing pumice saturated with sodium carbonate did not alter the result. To test the effect of heat, an apparatus was used consisting of a large U tube containing pumice and sulphuric acid, a narrow U tube, which could be placed in a test tube, a weighed tube containing pumice and sulphuric acid and a flask with potassium iodide and starch solution, acidified. From one to five liters of gas were drawn through the tubes very slowly, the narrow tube being cold, plunged in boiling water or in a paraffin bath at 150° to 200°. The second U tube was weighed after each experiment, and the starch solution was decolorized by a deci-normal solution of sodium thiosulphate. The maximum increase in weight in twenty-four experiments was ·0035 gram, in an experiment at ordinary temperatures, the decolorization requiring 3·65 c.c. of the thiosulphate. At 200° the sulphuric acid increased in weight ·0006 gram, the decolorizing solution used being 1·8 c.c. Since one c.c. of this solution corresponds to ·017 gram hydroxyl, which would yield on decomposition by heat ·009 gram water, and at 200° at least half the hydroxyl would be destroyed, an increase of ·016 gram instead of ·0006 gram might have been expected had hydroxyl been present. Moreover, contact with strong sulphuric acid did not render the gas inactive. Hence the author's conclusion that the gas formed during the slow oxidation of phosphorus is actually ozone.—*J. Chem. Soc.*, xxxvii, 118, Feb., 1880.

G. F. B.

2. *Explosion of a Platinum Alembic used for concentrating Sulphuric acid.*—KUHLMANN (fils) has communicated to the Chemical Society of Paris the particulars of the explosion of a platinum still used in his factory at Lille, for concentrating sulphuric acid. The still was 90 centimeters in diameter, and could concentrate 6 to 7000 kilograms of acid in twenty-four hours. The explosion scattered the fragments of it to a distance of twenty or thirty meters, tearing both the body and the head of the still in pieces, and throwing the bricks of the furnace violently about. Happily a slight hissing one or two seconds before, gave warning to the workmen who left the vicinity and escaped a terrible fate. The cause of the explosion seems to be as follows: The acid had nearly all been withdrawn from the still, a layer five centimeters deep, thirty or forty kilograms, being left in the bottom. Upon this water had been run by means of the syphon, and the whole had been heated slowly for several hours in order to clean the apparatus. It would appear that the acid and water did not combine at once, but that union took place subsequently at an elevated temperature and very rapidly, giving rise to a large production of heat. According to data given by Favre and Silbermann and others, one kilogram of acid added to a suitable

quantity of water, evolves 148 calories. Consequently the forty kilograms of acid would have evolved heat enough to give rise to an instantaneous production of eighteen or twenty cubic meters of vapor; a quantity quite sufficient to burst a platinum vessel of 300 liters capacity whose walls were only two or three millimeters thick. The experiment had been repeated by the author several times, using glass vessels. The explosion always took place with violence provided the water was at least ten molecules for each one of the acid. Pfaundler, using a considerable quantity of the materials, in the ratio of one molecule of acid and 120 of water, has obtained an evolution of 181 calories. The experiments were made, however, at 18°, and the author is examining the production of heat when the mixture takes place at higher temperatures.—*Bull. Soc. Ch.*, II, xxxiii, 50, Jan. 1880. G. F. B.

3. *On the Equivalence of Boron.*—In an investigation of the phenyl derivatives of the nitrogen series, Michaelis had shown that the free equivalence of a chloride appears distinctly if a phenyl group replaces chlorine. Thus phosphorous chloride, which unites difficultly with bromine, combines easily if an atom of phenyl replaces one of chlorine. So monophenylarsenous chloride takes up chlorine readily, while arsenous chloride does not combine with it. In connection with Becker, Michaelis has now studied monophenyl-boron chloride, hoping to obtain the same result, and to obtain a chloride in which boron is a pentad. By the action of boron chloride upon mercury-diphenyl, in sealed tubes at a temperature of 180° to 200°, monophenylboron chloride is obtained as a colorless liquid, easily becoming reddish, boiling at 175°, and solidifying at 0°. When this is placed in a freezing mixture and chlorine passed over it, it is absorbed and the mass liquefies. In the experiment the increase of weight was noted; and it was found that 3·9 grams had absorbed 1·3 grams of chlorine, theory requiring for the two atoms of chlorine absorbed, 1·7 grams. The authors think that these results render probable the existence of a phenyl-boron tetrachloride $C_6H_5BCl_4$ which breaks up easily into boron chloride and monochlorbenzene. Conceding this, boron is quinquivalent.—*Ber. Berl. Chem. Ges.*, xiii, 58, Jan., 1880. G. F. B.

4. *On the Direct Union of Cyanogen and Hydrogen.*—Berthelot has succeeded in causing cyanogen to unite directly with hydrogen under the influence of heat. The pure and dry gases, mixed in equal volumes and passed slowly through a narrow tube heated to 500°—550°, combined to an extent of two or three per cent. But if the action be prolonged, the mixture being contained in a sealed tube and heated to the above temperature for several hours, the two combine in equal volumes forming hydrogen cyanide free from cyanogen, only one-seventh of which is transformed into paracyanogen. The phenomenon differs from the synthesis of hydrogen chloride only in its greater slowness and the more elevated temperature required, which is that at which hydrogen unites directly with oxygen, with ethylene, etc.

If the temperature be lower, there is an excess of uncombined hydrogen; if it be very high, free nitrogen is produced. The hydrogen cyanide formed, however, remains intact. Experiments were then made with the metals, and it was found that cyanogen unites directly with them. At 300° zinc, cadmium and iron form cyanides of these metals. Zinc is attacked even in the cold after several days; at 100° in three or four hours. Copper and lead yield a trace of cyanide at 500° to 550°. Silver and mercury do not combine directly with cyanogen at any temperature; probably because the temperature of the reaction is also the temperature of decomposition.—*Bull. Soc. Ch.*, II, xxxiii, 2, Jan., 1880. G. F. B.

5. *On Cellulose and its Nitro derivatives.*—EDER has made an extended examination of the nitro-derivatives of cellulose with a view to determine whether they are compound ethers of nitric acid or are nitro-substitution products. He gives the following reasons for the former view: (1) Alkalies withdraw variable quantities of nitryl forming nitrates; (2) Sulphuric acid even in the cold, expels all the nitryl in the form of nitric acid, a sulpho-ether resulting; (3) Toward ferrous sulphate and chloride, these derivatives act in the same way as nitrates; (4) By treating them with sulphuric acid over mercury, they act like nitrates, all the nitrogen being evolved as nitrogen dioxide; (5) Reducing agents as potassium sulphydrate, sodium stannite, ferrous acetate, etc., convert them into ordinary cellulose. Hence the pyroxylins are nitrates of cellulose and have the general formula $C_{12}H_{20-n}O_{10-n}(O.NO_2)_n$, the formula of cellulose being $C_{12}H_{20}O_{10}$. The author has also examined the definite compounds formed by the action of nitric acid on cellulose. When the perfectly dry cotton is placed in a cooled mixture of three volumes pure concentrated sulphuric acid (1·845) and one volume nitric acid (1·5) for twenty-four hours, and after washing and drying, is treated with a mixture of three parts of ether and one part of alcohol until everything soluble is removed, there is left a compound having the composition of cellulose hexanitrate, and yielding about 14 per cent of nitrogen. By using less concentrated acids, definite compounds were obtained containing less nitryl; the pentanitrate giving 12·57 per cent nitrogen, the tetranitrate with 11·41, the trinitrate with 10·12 per cent (evidently containing tetranitrate) and the dinitrate with 6·89 per cent. All these are soluble in a mixture of ether and alcohol except the first given, the hexanitrate. The mononitrate was not obtained.—*Ber. Berl. Chem. Ges.*, xiii, 169, Feb. 1880. G. F. B.

6. *On a new kind of Ammonium Bases.*—GRIESS has described a new series of ammonium bases obtained by acting on the isomeric amidophenols with excess of methyl iodide. When to a cold solution of one part orthoamidophenol hydrochlorate in methyl alcohol, three parts of methyl iodide is added and then concentrated potash solution to strong alkaline reaction, and the whole is allowed to stand, an acid reaction appears after some

time. This is made alkaline again, and the operation is repeated as long as the solution becomes acid. On distilling off the methyl alcohol, the liquid solidifies to a mass of yellowish colored crystals of orthotrimethylphenolammonium hydriodate. The base is obtained from this by treatment with silver oxide, in well formed prisms, having the formula $C_9H_{13}NO$, or $C_6H_4<\overset{O}{\underset{N(CH_3)_3}{|}}$. Several salts of this new ammonium are described, beside the platinum double salt. By treating paraamidophenol in the same way, paratrimethylphenolammonium results.—*Ber. Berl. Chem. Ges.*, xiii, 246, Feb., 1880. G. F. B.

7. *Photographs of Spectra.*—H. W. VOGEL has photographed the spectra of oxygen, hydrogen and quicksilver, by means of the sensitive gelatine bromide of silver plates, introduced by Wratten and Wainright, which Vogel regards as fifteen times more sensitive than the most sensitive wet plates. With an exposure of two hours a spectrum from the green to the violet could be obtained by ordinary induction sparks, without the use of a Leyden jar. Full tables of wave-lengths of the lines, with descriptions of their character and list of their coincidences with solar lines, according to different authorities, are given. Vogel confirms the result previously obtained by Wiedemann, that if quicksilver and nitrogen are heated together in a Geissler tube the lines of nitrogen disappear when the tension of the quicksilver increases to a notable extent. From the fact that the H$\delta$ line appears in the Geissler tube at a pressure of $2^{mm}$ when single induction sparks are discharged through it, Vogel concludes that their existence is not due alone to very high temperatures, as Lockyer assumes. It was noteworthy that in the spectra in tubes the strong mercury line $\lambda=4046$ was entirely absent, while weak lines were present which were not seen with the employment of strong pressure and strong discharges: also by rarefaction and the consequent lowering of temperature, one of the brightest, and, according to Lockyer, longest lines disappears, while many weaker and less refrangible lines remain. Further, not all the lines increase in brightness, but many disappear. The assertion of Lockyer, that with diminishing pressure the shortest lines disappear first, is therefore not true in general.—*Ber. d. Berl. Ak.*, 1879, p. 586. J. T.

8. *The limits of the Ultra Violet in the Solar Spectrum at different heights.*—CORNU has made some spectroscopic tests on the Riffel, at Visp and on the Rigi, and finds results in accordance with those of previous observers. The ultra violet limit is very slightly increased with the altitude of observation. The change of wave-length is about a millionth of a millimeter for 700 meters. The limit at the Riffel was reached at a wave-length of 293·2, and at Visp at 295·4.—*Comptes Rendus*, lxxxix, p. 808, 1879. J. T.

9. *Atmospheric Polarization and the influence of Terrestrial Magnetism upon the Atmosphere.*—The apparatus of Becquerel consisted essentially of a Savart polariscope mounted upon a divi-

ded circle. This divided circle is supported by a horizontal axis which is also capable of turning about a vertical axis. Two other divided circles measure the motion in azimuth and in declination, and allow the polariscope to be directed in any direction, and give also the coördinates of the point of observation. The method of observation was to obtain upon the same divided circle the position of the plane of polarization of the sun and the trace of the plane of the sun. An ingenious method of accomplishing this is detailed in the author's memoir. The conclusions reached are as follows:

(1) The existence at one point of a variable angle between the plane of the sun and the plane of polarization of the atmosphere.

(2) A periodical variation in this angle, which has a maximum and minimum during the day. This phenomenon appears to be connected with the variable conditions of illumination of the atmosphere due to the height of the sun.

(3) The manifestation of the magnetic influence of the earth upon the atmosphere, to which influence can be attributed a small deviation of the plane of polarization of the light.—*Annales de Chemie et de Physique*, Jan., 1880, p. 90. J. T.

10. *Measurements and law in Electro-Optics.*—Dr. KERR continues his experiments upon the effect of electric tension on the transmission of light through dielectrics, and enunciates this general law:—"*The intensity of electro-optic action of a given dielectric, (or the difference of retardations of the ordinary and extraordinary rays) per unit of thickness of the dielectric, varies directly as the square of the resultant electric force.*" The dielectric used by Dr. Kerr was carbon disulphide, and was contained in a cell of peculiar construction, which allowed the layer of the dielectric to be submitted to powerful electric stress. A beam of light was passed through a Nicol prism and then through the layer, and was examined by a second Nicol. In subsequent experiments, Jamin's compensator was also used between the layer and the ocular Nicol. Thomson's long-range electrometer was employed to measure the electrostatic effects. The author concludes that Faraday's and Clerk-Maxwell's views in relation to the action of a dielectric in the transmission of electrostatic force, and the state of molecular constraint that is associated with and is essential to that action, are very strongly confirmed by the new facts of electro-optics. "The dioptric action of an electrically charged medium is closely related to the electric stress of the medium, the axis of double refraction coinciding in every case with the line of electric tension, and the double refraction varying, certainly in $CS_2$ and probably in all other dielectrics, directly and simply as the intensity of the tension."—*Phil. Mag.*, March, 1880, p. 157. J. T.

11. *Connection between the laws of diffusion and Thermodynamics.*—BOLTZMANN, in a valuable paper, discusses the phenomena of diffusion from the point of view of the mechanical theory of heat, and arrives at certain equations expressing the entropy. The equivalent transformation of work due to diffusion, and the maximum work which can be produced by the diffusion of gases at constant temperature.—*Wien. Ber.*, 78, 1878. J. T.

12. *Density of the Halogens at very high temperatures.*—Professor CRAFTS has repeated the experiments of the Messrs. Meyer, with certain modifications, and finds with them that the density of iodine vapor at the temperature obtained with the gas furnace of Perrot is two-thirds of the normal density, although he thinks their estimates of the temperature too high. In experimenting, however, with free chlorine he obtained values corresponding to the normal density of $Cl_2$.

More recently Victor Meyer has published additional experiments on the same subject, which indicate that while chlorine does not assume the abnormal condition corresponding to $\frac{2}{3}Cl_2$ below 1200 degrees, iodine vapor passes into a similar state at 1000 degrees. He confirms also the results of Crafts in regard to the density of chlorine gas, which had been previously prepared and introduced as gas into the heated flask, finding that under these circumstances the density corresponded approximately to $Cl_2$ at the same temperature at which the chlorine formed in the flask from $PtCl_4$ assumed the lower density corresponding to $\frac{2}{3}Cl_2$. The old term *nascent* is applied to chlorine in the last condition, and it is suggested that the difference between chlorine gas in these two states of density corresponds to the difference between oxygen gas and ozone at the ordinary temperature of the air. Victor Meyer has further determined the vapor density of bromine under the same condition as chlorine and iodine. He finds that $PtBr_4$ can be easily prepared, and that the bromine vapor formed by its decomposition "in statu nascenti" assumes at the high temperatures employed in these experiments the value 3·64, corresponding to $\frac{2}{3}Br_2$. Experiments to determine the density of free bromine vapor formed by dropping liquid bromine into the heated flask did not give accordant or satisfactory results. Meyer has met with similar irregularities in his experiments on the vapor density of water and other volatile liquids at a white heat, which he attributes to a mechanical cause, depending on the very sudden conversion of the liquid into vapor, which at such high temperatures takes place with explosive violence. Meyer hopes to overcome the difficulties presented in such cases, by using larger vessels, and the several articles published in the Berichte of the German Chemical Society for March 8th, pp. 391 to 408, are obviously only preliminary notices of an investigation still in progress. The idea that a chemical decomposition or dissociation has taken place in these experiments appears to be no longer entertained, and the anomalous densities observed, so far as they are real phenomena, are probably the result of allotropic modifications of the elementary substance similar to those already well known. J. P. C.

13. *Use of the Heliotrope for telegraphic purposes in triangulation.* Letter to J. D. Dana, from Capt. C. P. PATTERSON, Superintendent of the Coast Survey, dated Washington, Mar. 26, 1880. —Having noticed in *Nature* several references to the use of the sun for telegraphic purposes, I beg to say that the heliotrope—a

mirror with directive mounting—has been one of our regular field instruments in triangulation for forty years, and has been used on lines from 20 to 192 miles in length.

As a matter of interest, I send you the following extract from the annual report of Assistant George Davidson, in charge of the most important portion of the triangulation on the Pacific Coast. The stations named are along the western crest of the Sierra Nevada Mountains, along the crest of the immediate Coast Range, and Mt. Shasta, at the head of the Sacramento Valley.

I notice that M. Perrier, in charge of the triangulation in Algeria, in making his connections with Spain, failed—on account of the state of the atmosphere I presume—to obtain satisfactory results from the heliotrope (the distance being as nearly as can be learned 165 miles), and was obliged to have recourse to electric lights, requiring engines of 6-horse power. With these his success was perfect.

[EXTRACT.]—*Heliotrope Spectra.*

My former experience of the decomposition of the heliotrope image of the sun after passing through many miles of the atmosphere was fully verified. The heliotrope images were seldom decomposed on the lines under seventy miles in length, but they were, as a rule, decomposed on all longer lines, and ranged

| | | | | |
|---|---|---|---|---|
| | Blue | Blue | Green | Green |
| through the formulæ | White | Yellow | Yellow | Orange, the last |
| | Red | Red | Red | Red |

being the less frequently seen. The peculiar sparkling characteristic of the bright heliotrope image does not announce itself in the spectrum image; the colors give a steady, soft image, which is generally slightly higher than broad; frequently twice as high as broad; and upon some occasions it reaches a height of 60 seconds with a breadth of 10″ to 15″.

In nearly all cases the red is the most marked and is certainly the most persistent; the blue will fade away, the yellow or orange is not in sufficient contrast with the white field to be observed upon. The color of the red is that of the spectrum at the B line. Frequently the red will exist with a sharply defined nucleus of light orange, or even white; but it is doubtful if this exists when the spectrum is formed.

The column of spectrum light sometimes undergoes the most unceasing apparent interchange of the colors, as if the rapid changes in vertical refraction suddenly shortened and lengthened the column; and yet tests failed to show that any part of the column was not in the vertical.

It is only upon rare occasions that the spectrum is seen inverted, and then seldom during the single minute of pointing; but at Mount Lola and at Round Top Mountain the records show where the heliotrope, seen as a diffused disc, had the red and blue at different parts and in different proportions of the circumference with yellowish center. Upon the great lines the spectrum was occasion-

ally very brilliant in $\begin{matrix}\text{Green}\\ \text{Orange,}\\ \text{Red}\end{matrix}$ and then it was possible to predict its visibility to the unassisted eye.

From Mount Lola we have frequently seen the heliotrope images with the naked eye over the longest lines; and at Round Top Mountain, with lines reaching 160 miles, we saw the Snow Mount heliotrope very plainly with the naked eye upon several occasions. I am satisfied that the Mount Helena heliotrope would have been seen with the naked eye from Mount Shasta, 192 miles, in favorable weather. Although the spectrum on such occasions is very vivid, the naked eye did not detect the colors.

In a former paper I have referred to this phenomenon as a normal condition of the heliotrope image, but particularly exhibited in long lines. On short lines the difference of density of the stratum of air traversed by the ray of light is too small to decompose the white light; but on long lines the strata of different densities through which the line passes act practically as a prism by which the ray of white light is decomposed by the different refractive powers into the spectrum.

*Heliotropes.*—The heliotropes I use are of the pattern which I devised in 1851, except that now a square mirror is adopted as affording more light for the same sized box for transportation. It could be made a parallelogram, but would be less steady in a heavy wind. The larger ones have a screen attached to a lever for transmitting Morse telegraph letters as heretofore.

The ordinary amalgam-backed mirrors become whitish and opaque in a few days' exposure, but the fine silver deposit on the glass retains its brilliancy unimpaired.

I have established the following sizes for heliotropes, and have practically tested them up to 192 miles. I feel certain that the proportions may be carried to any practicable line on the earth's surface.

| Dist'ce. Miles. | Surface. Sq. inches. | Side. Inches. | Actually used. | Dist'ce. Miles. | Surface. Sq. inches. | Side. Inches. | Actually used. |
|---|---|---|---|---|---|---|---|
| 10 | 0·21 | 0·46 | — | 130 | 35·5 | 6·0 | |
| 13 | 0·36 | 0·60 | 0·4 | 138 | 40· | 6·3 | 5·6 too weak. |
| 20 | 0·84 | 0·92 | — | 141 | 42· | 6·5 | 6· weak. |
| 30 | 1·89 | 1·37 | — | 148 | 46· | 6·8 | 7· |
| 40 | 3·36 | 1·83 | 1·5 | | | | 6· v. weak. |
| 50 | 5·25 | 2·3 | 3·0 too bright. | 160 | 54· | 7·3 | 8· |
| 60 | 7·55 | 2·8 | 3.0 | 169 | 60· | 7.7 | 8· |
| 70 | 10·28 | 3·2 | 3·0 | 192 | 77·3 | 8·8 | { 8· |
| 80 | 13·4 | 3·7 | — | | | | { 12· Small telescope. |
| 90 | 17· | 4·1 | — | 200 | 84· | 9·2 | |
| 100 | 21· | 4·6 | — | 250 | 131· | 11·5 | |
| 107 | 24· | 4·9 | 5·0 | 300 | 189· | 13·8 | |
| 120 | 30· | 5·5 | { 5·0 weak. { 6·0 strong. | | | | |

Having established by experience the best size for a heliotrope upon a line of a given length, I obtained the following formula, based upon the general law of optics, for determining the size for any required distance: $x=d^2\times\cdot0021$, where $x$ is the area of the required heliotrope *in square inches*, $d$ the given distance in miles.

In the smaller heliotropes the amount of light cut off by the skeleton vane bears an undue proportion to the size of the mirror, and this must therefore be taken into consideration. I use the smallest skeleton vanes compatible with stiffness. If for any cause a larger heliotrope is needed on a given line, and under favorable circumstances the image appears too bright, the area of the mirror may be readily reduced upon a preconcerted signal, by affixing an open frame of tin or even of paper upon the outer edges of the mirror.

14. *On the Artificial Formation of the Diamond.*—The last number of the Proceedings of the Royal Society (No. 201), contains a preliminary notice by Mr. J. B. Hannay of the process by which he seems to have succeeded in obtaining crystallized carbon identical with the diamond. The following is an extract.

"When the carbon is set free from the hydrocarbon in presence of a stable compound containing nitrogen, the whole being near a red heat and under a very high pressure, the carbon is so acted upon by the nitrogen compound that it is obtained in the clear, transparent form of the diamond. The great difficulty lies in the construction of an inclosing vessel strong enough to withstand the enormous pressure and high temperature, tubes constructed on the gun-barrel principle (with a wrought iron coil), of only half an inch bore and four inches external diameter, being torn open in nine cases out of ten.

"The carbon obtained in the successful experiments is as hard as natural diamond, scratching all other crystals, and it does not affect polarized light. I have obtained crystals with curved faces belonging to the octahedral form, and diamond is the only substance crystallizing in this manner. The specific gravity is as high as 3·5. The crystals burn easily on thin platinum-foil over a good blowpipe, and leave no residue, and after two days' immersion in hydrofluoric acid they show no sign of dissolving, even when boiled. On heating a splinter in the electric arc, it turned black—a very characteristic reaction of diamond.

"Lastly, a little apparatus was constructed for effecting a combustion of the crystals and determining their composition. The ordinary organic analysis method was used, but the diamond crystals were laid on a thin piece of platinum-foil, and this was ignited by an electric current, and the combustion conducted in pure oxygen. The result obtained was that the sample (14 mgrms.) contained 97·85 per cent of carbon, a very close approximation, considering the small quantity at my disposal."

## II. Geology and Mineralogy.

1. *Sketches of the Physical Geography and Geology of Nebraska;* by Samuel Aughey, Ph.D., LL.D., Professor of Nat. Sci. in the Univ. of Nebraska. 326 pp. 8vo. Omaha, Nebraska, 1880.—These "sketches" contain a well-arranged and carefully prepared description of the region of Nebraska, as regards its topography, climatology, drainage and geology, and a general account of its flora and fauna. We cite the following facts from it:

The State has a length (maximum) from east to west, of 413 miles. The average elevation of the eastern half is 1,700 feet, of the western 2,612 feet. The mean elevation of the State is 2,312 feet. The ascent for 100 miles west from Omaha is $5\frac{1}{2}$ feet a mile; for the second 100, 7 feet; the third 100, $7\frac{1}{2}$ feet; the fourth, $10\frac{1}{2}$ feet.

The increasing size of the streams and the increasing number of springs during the past fifteen years indicates an increased rainfall. Professor Aughey attributes this greater amount of rains to the turning up of the soil for cultivation, which has rendered it, on an average, nine-fold (by his experiments) more absorptive of the water from rains. The water that falls on the hard original soil of the prairies mostly flows off into the canyons and streams; while the broken soil, like a huge sponge, takes it all in. At the time of the first settlement of the state the average rain-fall was 20 inches and the part absorbed probably not more than 5 inches; now it is 32 inches, and not less than 24 are absorbed. The great thickness of the soil—of all depths to 200 feet in the lœss regions—gives this sponge its great magnitude and power.

Professor Aughey discusses well the facts relating to the lœss and its origin. He shows that Richthofen's wind-drift theory finds no support in the character of the Nebraska deposits. He states that the Missouri river in the period when the lœss was being deposited must have been from 5 to 30 miles in breadth; and that the Platte, the Niobrara and Republican spread over their respective plains in the same way. He mentions the occurrence of stratification, and in some parts of thin lamination; of transitions into or alternations with sandy beds; in its lower part, the occurrence of fresh-water as well as land shells, and various other facts bearing on the question of origin. He speaks of examining the silt after a flood on the Missouri four miles below Dakota City, and of obtaining, in 1871, of existing kinds brought down by the river, 35 species of land shells and 20 of fresh-water species. The lœss of eastern Nebraska is over 3,000 feet below its highest point on the west line of the State. But this height, so far as not due to a small eastward pitch in the waters, is accounted for by a change of level.

The remains of life found in the lœss are, in addition to the molluscan, those of the rabbit, gopher, otter, beaver, squirrel, deer, elk and buffalo. Bones of the mastodon and elephant are

also often found, especially in Lancaster county. In Lincoln, situated "near the western shore of the Missouri lake of the period," "they have been found in at least twenty wells." Professor Aughey states that in 1872 he found in the lœss, three miles east of Sioux City, in a railroad cut, an Indian arrow-head; and he has since found arrow-heads, spear-heads and flint-chips at other points: in the bluffs back of Jackson, in 1874, 2½ miles southeast of Omaha, in a railroad cut, 15 to 20 feet below the top of the lœss; east of the Republican Forks, 14 feet below the surface, underneath the second bed of black soil. "It appears then that some old races lived around the shores of this lake, paddled their canoes over its waters, and accidentally dropped their arrows in its waters or let them fly at a passing water fowl." The rising of the land "closing the lœss period first clearly outlined the present rivers of Nebraska."

At the present time the rivers have very wide river-bottoms, ranging from one to ten miles or more. Above these there are one or more terraces: a lower, usually 3 to 6 feet above the river-bottoms; a second, 12 to 25 feet; and a third, of varying elevation. The bottom-lands are seldom overflowed in any part at the highest floods.

2. *Silurian age of the Crystalline rocks of Eastern Pennsylvania and Hudson River age of the Hydromica Schists.*—Mr. Charles E. Hall has an important paper on this subject in the Proceedings of the American Philosophical Society for January, 1880, together with a map illustrating it. He first mentions the occurrence of the fossil seaweed *Buthotrephis flexuosa*, of probably Hudson River Age, in the Peach Bottom roofing slates of York County, Pa., announced by Prof. Frazer. The rocks of the region in order of age are stated to be:

(1) Granitoid, syenitic, hornblendic, micaceous and quartzose rocks, older than Silurian, that is, of Archæan or Azoic age: one division extending in a southwesterly belt across Bucks and Montgomery Counties, as far west as Chestnut Hill, Philadelphia; a second, from the vicinity of the last mentioned locality, westward across the Schuylkill, and covering much of northern Delaware County.

(2) Potsdam sandstone and conglomerate, with some schistose beds resting unconformably on the preceding; over these, dolomites, crystalline limestones (the dolomites below the limestones) hydromica schists, the upper part of which group has afforded Trenton fossils at Buckingham, Bucks County; and also hydromica schists, quartzose schists, chloritic schists, with occasional beds of quartzite and serpentine, which are of the Hudson River group, and flank the Chester Valley on the south from some point not far east of the Schuylkill River throughout the entire length of the valley; and micaceous garnetiferous schists with limestone, in the southern central portion of Chester County, resting upon the last-mentioned hydromica schists. These Hudson River schists are those that have been called Taconic. Details are given

illustrating the conformability of all these schists and limestones. Together they occupy the region between the Archæan and the Triassic area.

(3) Mica schists, hornblendic, garnetiferous and talcose schists of Philadelphia, with soapstone and serpentine, which are described as resting unconformably on the preceding. A large part of this area is on the southeast side of the Archæan between it and Delaware River. The nonconformity "may be owing to faulting, but they [the rocks] are nevertheless more recent."

The serpentines of Radnor township in Delaware County, and those of Easttown, Willistown, East and West Goshen are spoken of as "undoubtedly altered beds of the South Valley Hill slates or Hudson River slates." The serpentines of Lancaster County are referred to the same age, and also probably those of Chester County.

3. *On the Carboniferous Volcanic Rocks of the Basin of the Firth of Forth: their structure in the field and under the microscope;* by ARCHIBALD GEIKIE, Director H. M. Geol. Survey of Scotland. 82 pp. 4to, with three plates, containing a geological map; sections of strata, and microscopic views of slices.—This elaborate paper, after its historical introduction, presents an account of six "volcanic districts" within the region of the Basin of the Firth of Forth, and describes in detail the positions and relations of the beds, and the character of the rocks. The eruptive rocks are divided into (1) Augite-feldspar rocks (divided into diabases or the granitoid variety, dolerytes and basalts); (2) Olivine augite-serpentine rocks, seen only at two localities; (3) Feldspar-magnetite rocks, or the porphyrites, which played an important part among the earlier eruptions of the Carboniferous period; and (4) Felsitic rocks, constituting a few dikes; and besides these there are fragmental rocks or tufas.

4. *Contributions to the Geology of Eastern Massachusetts;* by WM. O. CROSBY. Occasional Papers of the Boston Society of Natural History, Vol. III. 288 pp. 8vo, with five plates and a colored geological map.—Mr. Crosby's papers are very important contributions to the geology of Eastern New England. The rocks are described in detail, both as to their kinds and distribution, and facts are given bearing on the question of age, and illustrating many points in geological science. Among the interesting questions discussed is that of the age of the conglomerate, associated with the Primordial slate, the area of which two formations extends three to four miles north of Boston, eight to ten west (and in lines still further), and six to seven miles south. Mr. Crosby concludes that the conglomerate probably underlies the slate. He remarks that the conglomerate contains pebbles of the Shawmut amygdaloid, a rock associated with it to the southwest. The maximum thickness of the conglomerate is made about 1,000 feet, and that of the slate nearly the same. Both are stated to be unconformable with the crystalline rocks adjoining, which are referred to the Archæan.

5. *Eleventh Annual Report of the U. S. Geological and Geographical Survey of the Territories, embracing Idaho and Wyoming, being a report of progress for the year* 1877; by F. V. HAYDEN, U. S. Geologist. 720 pp. 8vo, with numerous plates, seventy of them illustrating rock sections, topography, scenery, tufa formations, and ten of fossils, besides six of maps. Department of the Interior. Washington, 1879.—This thick volume, just out of press, bears evidence of the great activity and success in the explorations carried forward under Dr. Hayden. It is made up of a general report by Dr. Hayden, reviewing the work of the year; special geological reports by F. M. ENDLICH, O. ST. JOHN, and Dr. A. C. PEALE; paleontological, by C. A. WHITE; and topographical, by A D. WILSON and HENRY GANNETT.

With more space at command, large citations would here be made from the volume.

6. *Earthquake of San Salvador, of December* 21–30, 1879.—A recent letter from Mr. W. A. Goodyear, now Director of a Governmental Mining and Geological Survey of San Salvador, states that more than 600 earthquake shocks were felt there within the last ten days of 1879. They were heaviest about Lake Ilopango, where he was on the 23d and until the 27th, when a shock came, as he writes, "that broke the telegraph wire, and made the ground on which we stood a perfect network of cracks, opened new springs of water, increased the rivulets in the vicinity to ten times their volume, muddied the waters of the lake in many places, and rolled hundreds of thousands of tons of rocks and debris down the steep hills in the shape of land slides." "As a sequel to the earthquakes, on the night of January 20th and 21st a new volcano appeared in the center of the Lake of Ilopango. This volcano does not yet (Feb. 1st), show more than four or five acres of new rocks above the water, and has not yet discharged any liquid lava. But it is growing, and its clouds of steam are seen every day from the Capital, rising 1,000 feet or more above the lake."

7. *Oil-sands of the Bradford or Northern oil-district of Pennsylvania compared with those of the Venango or Western District.*—Mr. C. A. ASHBURNER, of the Geological Survey of Pennsylvania, states, in a paper read before the American Philosophical Society on March 5, that the oil-sands (or sandrocks) of the Bradford region are about as coarse as ordinary beach-sand, even in grain, of constant thickness, and widely spread, while those of the Venango district vary from coarse to fine, and abruptly also in thickness and composition; the bottom layer, which is the most productive, is in part a fine micaceous mud-made rock, yielding only a trace of oil. The former are of the Chemung group, the latter of the Red Catskill (No. IX of the Pennsylvania survey). In the Bradford district during the year 1879, 2536 wells were drilled to the Bradford oil-sand, of which only 3 per cent were *dry holes*, while in the Venango district, during the same year, 475 wells were drilled (in the Counties of Warren, Venango, Clarion and Butler), and 122 were *dry holes*, or 25·7 per cent.

The largest *individual* wells have been opened in the Venango district, but the average yield has been greatest in the Bradford; some of the former having produced 2,000 to 3,000 barrels of oil per day, while the largest of the latter have not exceeded as many hundred.

8. *Giesecke's Mineralogiske Rejse i Grönland*, ved F. Johnstrup; med et Tillæg om de Grönlandske stednavnes Retskrivning og Etymologi af Dr. H. Rink. 372 pp. 8vo, with 3 plates. Copenhagen, 1878.—This work gives the diary of Carl Ludvig Giesecke during his travels in Western Greenland from 1806 through 1813, or about eight years. Giesecke was the first to call attention to the mineralogical and geological nature of the part of the country inhabited by Europeans, and this gives an especial interest to his observations. His travels extended along the western coast between latitude 60° and 73° north, and although his attention was especially directed to the collection of minerals and rocks, he devoted much time also to the geography and ethnography of Greenland. Among the new minerals discovered by him are eudialyte, arfvedsonite, allanite, gieseckite, fergusonite, sapphirine; he also described the method of occurrence of cryolite. During his life he published but little, and it is only now that his diary, which was sent each year to Copenhagen as a report of the results of his investigations, has been published.

9. *Explorations in the Interior of Greenland in* 1878; by Lieutenant Jensen. (Meddelelser om Grönland udgivne af Commissionen for Ledelsen af de geologiske og geographiske Undersögelser i Grönland; part I, 195 pp. 8vo, with 6 plates and 3 charts. Copenhagen, 1879.)—This volume contains an account of the explorations carried forward in Southern Greenland, (lat. 62° 15′ to 64° 30′) by Lieut. Jensen and his associates during 1878. His labors form a part of the general plan for the geological exploration of Western Greenland adopted by the Danish Government in 1876 and prosecuted actively since that time; a work in which the geologist Steenstrup has taken a prominent part, especially in Northern Greenland.

A summary of the results of Lieut. Jensen and of the relations of these explorations to those which preceded him, from the time of Giesecke down (see above) is given at the close of the volume by Prof. Johnstrup. From it the following facts are extracted. The continental ice of Southern Greenland has been hitherto an almost absolutely unknown region, the only extended journeys upon it are those of Dalager in 1751, and of Nordenskiöld in 1870. Lieut. Jensen, with great courage and perseverance, has succeeded in penetrating a distance of about forty-five English miles from the coast, starting from the "Frederickshaab Isblink" (lat. 62° 30′). A peculiar interest attaches to the expedition since at the farthest point of the journey inland, he reached a series of isolated rocky summits, emerging like islands above the continental ice (they are called *Jensen's "Nunatakker*). From these peaks, 5000 feet above the sea level, and

surrounded now, as they must have been for a vast period, by the fields of continental ice, a large collection of plants (fifty-four species) was obtained, and some few animals were also found. The ice on the east side of the "Nunatakker" has an altitude of 1570 meters above the sea, and must have a considerable thickness, since from the western edge it rises inland at a small angle (0° 49′). These isolated peaks exert a decided influence on the movement of the ice, so that in places its direction is reversed. In the midst of the continental ice the planes of dislocation are nearly perpendicular to the surface, but on the edges and about the "Nunatakker" they are inclined where the melting is greater and there results an increase in velocity in the upper parts of the ice. The crevasses are in part perpendicular and in part parallel to the direction of the movement, according to the nature of the surface underneath, and whenever the ice can expand fan-shaped they are radial and tangential. In the neighborhood of rocks, both of the interior peaks and of the coast, the surface of the continental ice is impregnated with fine clay and sand which are brought upon it by the tempests, and which the streams carry off into cavities of the ice mass. At one spot on the edge of the ice a pyramid of ice, occasioned by this accumulation of clay, was observed which was nearly sixty feet high. Moraines of different forms were noted on the ice in the region of the "Nunatakker" and they are considered as ground and terminal moraines. They form often curved and semi-circular lines and consist of much rounded blocks of stone of no great size. These, as they advance, fall into the crevasses, and thus the moraine disappears.

This volume contains a record of the astronomical and meteorological observations made during the expedition; also notes upon the Geology of the portion of Greenland explored by M. Kornerup; and a list of the plants collected, this last is given by M. Lange. The colored plates give a vivid impression of the great ice field over which the party traveled.

10. *Geological Chart of Belgium and the neighboring Provinces;* by A. Dewalque, Brussels.—This colored geological chart of Belgium measures twenty-four inches by twenty. It is a beautiful specimen of chromo-lithography. The areas of nearly fifty different formations are represented by colors and etching, ranging from the Pliocene through the whole geological series to its bottom, and including also some eruptive and other crystalline rocks.

11. *Edible Earth from Japan;* by E. G. Love, Ph.D. (Communicated).—It is somewhat surprising to find that the Japanese, in part even, are addicted to the eating of earth. The following analysis shows the composition of an earth eaten to a considerable extent by the Ainos. The clay, occurring in a bed several feet in thickness, is found in the small valley of Tsietonai (eat-earth-valley) on the north coast of Yesso. It is of a light gray color and very fine in structure.

| $SiO_2$ | $Al_2O_3$ | $Fe_2O_3$ | $Mn_2O_3$ | CaO | MgO | $K_2O$ | $Na_2O$ | $SO_3$ | $P_2O_5$ | $H_2O$* |
|---|---|---|---|---|---|---|---|---|---|---|
| 67·19 | 13·61 | 1·11 | 0·07 | 3·89 | 1·99 | 0·23 | 0·75 | 0·19 | *tr.* | 11·02 = 100·05 |

* With volatile matter.

The "volatile matter," which was very small in amount, consists of the fragments of the leaf of some plant which the people intentionally mix with the clay for the aromatic principle it contains, or which was accidentally picked up when the specimen was taken. On treating the leaf with ether and evaporating, I obtained some oily matter of an agreeable odor, but too small in amount to enable me to determine its nature. The Ainos think the earth contains some beneficial substance, and eat it on this account and not because it is a necessity with them. They have meat and an abundance of vegetable food. The clay is eaten in the form of a soup. Several pounds are boiled with lily roots in a small quantity of water and afterwards strained. The Ainos claim that the soup thus prepared is very palatable.

12. *On Conodonts, etc.*—The author of the monograph on Conodonts, quoted from in the last (April) number, p. 327, is Mr. George Jennings Hinde, not Hyde as there printed.

## III. Botany and Zoology.

1. *Genera Plantarum ad Exemplaria imprimis in Herbariis Kewensibus servata definita;* auct. G. Bentham et J. D. Hooker. Vol. iii, part I, pp. 459 (including temporary index). London, Reeve & Co., &c., 1880.—The first part of the first volume was issued in the summer of 1862. In February of the present year, the portion of the third and last volume containing the Monochlamydeous Dicotyledons and the Gymnosperms makes its appearance, so we may hope that the completion of the Phanerogamia, with which this great work may be expected to close, is not far distant. But the death of General Munro, who would have helped greatly in the grasses, is a drawback. It should be understood, however, that no order and no group of plants whatever has been taken from the hands of any collaborator or monographer. The work has all been done from nature, at first hand by the authors themselves,—a thing that has never been done before in a Genera Plantarum, since that of Jussieu, and hardly then.

This notice may be confined to indication of changes, such as may concern North American botany.

The monochlamydeous (otherwise called apetalous) series of orders begins with the *Nyctagineæ*. The leading genus *Mirabilis*, being still allowed to embrace *Quamoclidion*, their great difficulty was in keeping it clear of *Oxybaphus*. In this country we had regarded the anthocarp as most distinctive; but the present work prefers the form of the perianth and the texture of the involucre, but with hardly an improved result. *Senkenbergia* (*Tinantia*), which we had reduced to *Bœrhaavia*, is well restored.

The *Illecebraceæ* (*Paronychieæ*), excluded from their association with the *Caryophyllaceæ*, here find their natural place next the *Amarantaceæ*, and *Achyronychia*, which is referred to it, has a second species, one of the new things in the recent Mexican collection of Parry and Palmer.

In the succeeding order, *Amarantus* is restored to its old limits, including not only *Euxolus* and *Mengia*, but also *Amblogyne*. *Acnida* properly takes in *Montelia*, and a bad oversight of Gray's Manual is corrected. As was to be expected, *Cladothrix* of Nuttall takes generic rank.

*Chenopodiaceæ* are considerably extended, by some genera which were ambiguous between this and the preceding order, and still more by the inclusion of the *Baselleæ*. *Sarcobatus* forms a subtribe. The old difficulty of limiting *Chenopodium* and *Blitum* is effectually surmounted by reducing the latter to one of the five sections of the former. *Teloxys* is of course a *Chenopodium*, and *Obione* is not distinguished even sectionally from *Atriplex*. The important distinction between the plane-appressed fructiferous bracts of *Atriplex* and its allies, and the conduplication and union of these bracts in *Suckleya* and the three genera associated with it upon the establishment of the latter genus, is not as prominently exhibited as it would have been by the adoption of the subtribe *Eurotieæ*.

*Batis*, with a single species, stands for an order *Batideæ*.

*Polygonaceæ* are marked by the introdnction, between the *Eriogoneæ* and the *Eupolygoneæ*, of a tribe *Kœnigieæ*, which besides the typical genus and its evident relative *Pterostegia*, is made to include *Lastarriœa*, *Nemacaulis*, and *Hollisteria*, whose affinities are more Eriogoneous. Sereno Watson's natural arrangement of *Eriogonum* is adopted; but *Centrostegia* is kept distinct from *Chorizanthe*.

The *Piperaceæ* include the *Saurureæ*, and *Anemopsis* is reduced to *Houttynia*.

The great order *Laurineæ* is thoroughly revised; but all that concerns us is, that our Californian Laurel is removed from *Tetranthera* or rather *Litsea* Lam., and one of the two generic names which Nuttall applied to it is used for this at length well characterized genus, viz: *Umbellularia*,—not a good name, but it will serve.

In *Santalaceæ*, the still incompletely known genus *Darbya* is well removed from *Comandra*, to which DeCandolle referred it: whether it is here rightly joined to *Buckleya* can be known only when female flowers or fruit are discovered. These are special desiderata. The plant (a low shrub) is to be sought between Lincolnton, North Carolina, and Macon in Georgia!

The immense order *Euphorbiaceæ* has been studied anew, the great labors of Baillon and of J. Müller duly weighed, and the result is that six tribes are admitted under simple characters, and the ample sixth tribe, *Crotoneæ*, is divided into eight subtribes. The views adopted have been expounded by Mr. Bentham in a memoir, some notice of which has appeared in this Journal. The genera nearly reach 200, and there are over 3000 described species. We have soon to add a North American representative of the *Stenoldieæ*, and of the biovulate division, connecting with the *Phyllantheæ Reverchonia* of northwestern Texas and adja-

cent Arkansas. The male flowers of it are still wanting. Nuttall's *Alphera* is a section of the genus which is here given as "*Argithamnia* Swartz, Prodr.," but which begins as *Argythamnia* of Patrick Browne, a contemporary of Linnæus.*

The order *Urticaceæ* is adopted in the older and extended form, including the *Ulmeæ*, *Celtideæ*, *Cannabineæ*, *Moreæ*, *Artocarpeæ*, etc., as tribes. A main ojection to this view is surmounted by regarding the pistil in all of them as monocarpellary, and the two stigmas of Ulmus and the like as divisions of one or half-stigmas, after the analogy of many *Euphorbiaceæ*. *Ficus* is restored to its original proportions.

The ambiguous or anomalous genus *Leitneria* of Chapman, of which there is said to be a second species in Texas, stands as an order *Leitnerieæ*, which we crave leave to write *Leitneriaceæ*.

The *Cupuliferæ* are made to include not only the *Coryleæ* but the *Betulaceæ* also—which seems to be going too far.

*Empetraceæ* and *Ceratophylleæ* are among the supplementary orders which close the series. The compound pollen-grains of the former, after the Ericaceous type, have not been noticed.

The *Gymnospermeæ* consist of the *Gnetaceæ*, *Coniferæ*, taken in the largest sense, and *Cycadaceæ*. This notice is already too long to admit of an analysis of the treatment of the most important order *Coniferæ*, which is to a good degree original; and some points need to be pondered, before pronouncing an

* The history of this name and of its changes is curious and raises a nice point in the application of the rules of nomenclature. Patrick Browne founded it in the year 1756, wrote the name *Argythamnia*, but gave no etymology. It is pretty clearly inferable that he had ἀργυρος in view, that he should have written *Argyrothamnia*. We suppose that he shortened it in a way at that time and since not very uncommon, remembering the warning of Linnæus against *verba sesquipedalia* Unnecessary as it was in this case, it was a trivial curtailment compared with Rafinesque's *Nemopanthes*, razeed from *Nemopodanthes*, which no botanist has attempted to restore to its full proportions. Adanson adopted the genus under Browne's name in 1763. So did Swartz in his Prodromus in 1788.

*Argithamnia*, however, is the form adopted by Swartz in his Flora, in 1797, remarking that Browne derived the first part of the name either from ἀργός, white, or from ἀργυρεος, silvery. But if from the latter, Swartz should have written *Argyreothamnia*, if from the former *Argothamnia*.

Acting doubtless on the principle that if the orthography of a name might be changed to make it correct, it might be further changed to render it quite correct, Sprengel in his turn wrote it *Argothamnia*, and Mueller of Argan, *Argyrothamnia*. Now all these changes from first to last violate the rule (which is not without exceptions) that botanical names should be retained in their orginal form. At least mere improvement is no warrant for alteration. Mistakes may, indeed, be corrected. Thus Nuttall's genus *Wisteria*, in honor of *Dr. Wistar*, was properly corrected to *Wistaria*, in conformity with the rule that personal names should retain their orthography as nearly as possible. But upon our theory *Argythamnia* was not a mistake. Bentham and Hooker have acted upon the principle of preserving the original orthography; only they took the genus to originate with Swartz, passing by Browne, evidently because he did not use Linnæan specific names, though that could not affect the worth of his genera. If they had adopted the genus from Browne's original, or from Adanson who took it up in 1763, or from Swartz's Prodromus (1788), or from Jussieu in 1789, eight years before Swartz in his Flora wrote *Argithamnia*, we cannot doubt that they would have held to the original form, *Argythamnia*.

opinion. Suffice it to remark that the three classical suborders are suppressed (our authors are not fond of suborders), and the whole are disposed under six tribes, the leading characters of which are taken from the direction of the ovules. Two of them, the *Podocarpeæ* and the *Araucarieæ*, which, like the *Abietineæ*, have reversed ovules, do not here concern us.

*Thuya* is written in this unclassical form in which it came into botany through the old herbalists, adopted by Tournefort and Linnæus. It is made to include not only *Thuyopsis* and *Biota*, but even *Chamæcyparis* (*Retinospora*, etc.), which the arboriculturists will hardly like. We must wonder it was not extended to *Libocedrus* also, at least to the species of the northern hemisphere.

*Taxodium*, we are glad to notice, takes in *Glyptostrobus*. It is also gratifying that *Sequoia* goes into the same tribe, on the strength of the ovules being ascending or at least porrect at the outset, though the seed is reversed. The noteworthy fact comes to light that *Athrotaxis* of Tasmania is very nearly related to *Sequoia*. It should not be surprising that a type so early developed and widely diffused of old should have representatives in the southern hemisphere.

In the *Abietineæ*, the received genera are *Pinus* in the restricted sense, *Cedrus*, *Picea* (for the Firs, i. e. Balsam Firs), *Tsuga* (Hemlock Spruces and *T. Williamsonii*), *Pseudotsuga* (the Douglas Spruce), *Abies* (the true Spruces), *Larix*, the Larches. To the latter, rather than to *Cedrus*, *Pseudolarix* is appended, awaiting a knowledge of its male flowers.

Many and profound are the thanks due from all systematic botanists to the authors of this Genera Plantarum. A. G.

2. *On the Popular Names of British Plants, being an Explanation of the Origin and Meaning of our indigenous and most commonly cultivated Species.* By R. C. A. Prior, M.D., etc. Third Edition. London, F. Norgate, 1879. pp. 294, 12mo. —This book, full of interesting and curious lore, must have had the success it merits, for it is now passed to a third edition. The preceding one was issued in 1870. There is no specification of the changes which have been made in it, and the increase in size is of only four pages. But the scrupulous carefulness of the author warrants the assumption that it has received all needful supervision.

What we here need is a supplement to Dr. Prior's volume, recording the changes which have occurred in the application of English popular names to North American plants, and giving the history and application of our indigenous plant-names. A. G.

3. *Botanical Necrology:*—

John Carey—of whom few of the botanists of our day can have a personal remembrance—died at Blackheath, near London, March 26, ult., in the 83d year of his age. He came from London to the United States, in the spring of 1830, accompanied by three young and motherless children and by his brother, Samuel

T. Carey, who was also addicted to Botany. Both, we believe, were Fellows of the Linnean Society, and were near friends of Thomas Bell, afterwards the president of that society, who also lived to a good old age, dying only a few weeks earlier than the subject of this notice. Samuel T. Carey remained in the city of New York, in active business, and so was only an amateur botanist. His brother John, went into the country, first to Towanda, in the northern part of Pennsylvania, then to Bellows Falls, Vermont, where, giving much of his leisure to botanical pursuits, he resided until the year 1836, when he removed to New York, upon the entrance of his sons into Columbia College. He did not enter into business, but his administrative talents and great worth were so appreciated that he was at various times called to very responsible temporary positions. These positions, although unsought, were not unwelcome, for no small part of the moderate property he brought from England had been lost in investments made through reliance upon the honor and probity of defaulting States.

From the time of his arrival in the United States down to the year of his return to England in 1852, most of his leisure was given to botany, and much of it in companionship with the present writer, who was generously and greatly assisted by him in many critical studies. The proofs of the writer's first botanical book were revised by him; and to the first edition of the Manual of Botany Mr. Carey contributed the articles on *Salix* and on *Carex*, at that time the two most difficult parts of the work. In the year 1841 the two made a botanical journey together into the mountains of North Carolina, extending to the Grandfather and to the Roan, though a mishap upon the former mountain prevented Mr. Carey from reaching the latter. After the establishment of the writer at Cambridge, Mr. Carey was his frequent guest and an invaluable companion. His botanical career may be said to have closed in the year 1852. In that year he returned to England, alone, having successively lost his aged mother and his two younger sons, and seen the older son happily established in marriage. He engaged for several years in business, in connection with a friend of his youth, whose daughter he soon married, but lost within three years, after the birth of the second of two children, the solace and comfort and care of a serene old age, who survive to perpetuate his name, as we trust, on that side of the ocean also. Mr. Carey's first herbarium was destroyed by a calamitous fire in New York, at the time of the death of his youngest son. American botanists vied with each other in the endeavor to repair this serious loss, and another large collection of United States plants was formed, critically studied, and carefully annotated. This was presented to the Kew Gardens Museum eleven years ago. Several species of United States plants commemorate this honored name, among them a Saxifrage, which was discovered upon the excursion to the mountains of North Carolina, where the survivor of the party re-collected it last summer. The almost sole survivor of a botanical circle, of which Torrey was the center,

sadly but serenely pays the tribute of this brief note to the memory of a near and faithful friend, an accomplished botanist, a genial and warm-hearted and truly good man. A. G.

Coe F. Austin died at his home in Closter, New Jersey, on the 18th of March, ult., in the 49th year of his age. He was one of the original members of the Torrey Botanical Club, in which he will be much missed, and he was for some years, during its founder's lifetime, the curator of the Torrey herbarium. He was a very zealous and sharp-sighted botanist, and he followed his bent and pursued his investigations under many difficulties and privations. He contributed the article *Lemnaceæ* to Gray's Manual in 1867, has described several new species of Phænogamous plants; but has for a long while devoted himself most untiringly to Muscology, in which he was an adept. In a knowledge of *Hepaticæ* he was unrivalled in this country, and he fortunately enjoyed good opportunities for their study, the late Mr. Sullivant having long ago made over to him all his books and specimens relating to this order. He was to prepare a manual of the North American Hepatic Mosses, and perhaps has in good part done so, though it is to be feared that it is not completed. His last botanical work, finished upon the eve of fatal illness, was the description of the *Hepaticæ* of California for the second volume of the Botany of California, now in press. Several years ago he published *Exsiccati* of the Mosses of the Atlantic United States, and later sets of our *Hepaticæ*, embodying an immense amount of labor in their collection—in travel, mostly on foot, through the Southern, Eastern and Middle States—no less than in their investigation. We believe that these sets remain in good part unsold; and it is hoped that they will now be offered to botanists and secured by them to their advantage, and to the benefit of the family of the deceased. No botanist ever had a quicker eye for the detection of differences than Mr. Austin, or a more unreserved devotion to a favorite pursuit. A. G.

4. *On a collection of Crustacea from Virginia, North Carolina, and Florida, with a revision of the genera of Crangonidæ and Palæmonidæ;* by J. S. Kingsley. (From Proc. Acad. Nat. Sci. Phila. for 1879, pp. 383–427, pl. 14. No date: received March, 1880).—This, the longest of Mr. Kingsley's papers on American crustacea, notices about 100 species (9 of which are described as new), and is the most complete list yet published of the crustacea of the coast of the Southern States. It is based upon collections made by Prof. Webster of Union College. Decapoda only are included and of these the Pagurioidea are omitted. The paper covers partially the same ground as Mr Kingsley's "List of the Decapod Crustacea of the Atlantic coast, whose range includes Fort Macon" (op. cit., 1878, pp. 316–330), and is a marked improvement upon it. Attention should be called, however, to a few of the mistakes noticed in a cursory examination. In extending the range of *Leptopodia sagittaria* to Chili on the authority of A. Milne Edwards' identification of *L. debilis*

with that species, the author overlooks Milne Edwards' statement in the same paragraph that *L. sagittaria* Edwards and Lucas is a distinct species for which the name *modesta* is proposed. *Actæa spinifera* (sp. nov.) appears to be *A. acantha* A. Milne Edwards, which has been well figured twice; and if not Milne Edwards' species it should have been compared with it rather than with *A. hirsutissima*. *Eupilumnus Websteri* (gen. et sp. nov.), figured and very briefly described from a single specimen wanting the chelipeds, is evidently not very closely allied to *Pilumnus* and is apparently based on a young specimen of *Domacea hispida*, which had already been reported from the Florida reefs by Stimpson. Moreover, the name *Eupilumnus* is preoccupied, having been used (according to the Zoological Record for 1877) by Kossmann for a division of the old genus *Pilumnus*. In attempting, in a footnote on p. 405, to "straighten the synonymy of two species of *Petrolisthes*," the confusion in the synonymy of one of the species is increased. *Petrolisthes Helleri* is proposed for *Porcellana Danæ* Heller (non Gibbes), regarded by Heller as the same as *Porcel. armata* Dana (non Gibbes). Dana, however, discovered that his name *armata* was preoccupied and, in the appendix to his great work, p. 1593, and in the explanation to the plates, substituted *spinuligera* for his species, though this has been overlooked by Stimpson and Heller as well as by Kingsley. The reason for the reference of the species to *Petrolisthes* is not apparent, for Stimpson retained Dana's species in the restricted genus *Porcellana* and, at least, it has no appearance of being a *Petrolisthes*.

Under Caridea there is a useful revision of the genera of Crangonidæ, Atyidæ, and Palæmonidæ, though one is occasionally left in doubt as to the limits of the genera adopted; as in the case of the first genus, *Crangon*, which is said to include *Steiracrangon* Kinahan, while no mention whatever is made of the same author's *Cheraphilus*, which has recently been adopted by G. O. Sars and by Miers. A peculiar misuse of "ibid.", which the proof-reader ought to have corrected, might be overlooked did it not recur so persistently in nearly all of Mr. Kingsley's papers. S. I. SMITH.

5. *The Crayfish: an Introduction to the Study of Zoology*; by T. H. HUXLEY. 371 pp. 8vo. New York, 1880 (D. Appleton & Co.)—This last volume of the International Scientific Series is far more interesting than ordinary text-books of zoology and well-deserving of careful study. Though it treats specially of the natural history, physiology, morphology, comparative morphology, distribution, and origin of crayfishes, it admirably fulfills the author's desire, as expressed in the preface, "to show how the careful study of one of the commonest and most insignificant of animals, leads us, step by step, from every-day knowledge to the widest generalizations and the most difficult problems of zoology." A large part of the excellent wood-cut illustrations are new, and many are unusually beautiful for a work of this class. The figures (after Bate) on page 282, are of *Carcinus mænas*, not *Cancer pagurus* as labeled. S. I. SMITH.

## IV. Miscellaneous Scientific Intelligence.

1. *On the movement of Glaciers.*—A series of careful measurements has been made by K. R. Koch and Fr. Klocke on the Morteratsch glacier in Eastern Switzerland. Their object was a somewhat different one from that of previous observers, as they desired to determine whether in its general downward course the movement was uniformly forward, or whether discontinuous or at times backward. The investigations were limited to the observation of the movement of a point of the surface in a vertical plane parallel to the length of the glacier. The method employed was as follows: Two scales, perpendicular to one another, were so attached to the glacier that one stood vertical and the other horizontal, and their movements in their respective directions were noted by means of a fixed telescope at a distance.

The observations were carried on in the day time from August 28 to September 6 (1879). The station was situated on the west side of the glacier, about a mile from its lower end. It was favorably placed, both since it afforded a perfectly firm base for the telescope, and also because as at this spot the mass of the glacier is without any very considerable crevasses, either longitudinal or transverse, it was likely to be free from accidental variations. The stake to which the scales were attached was imbedded a foot and a half in the ice, and to prevent any melting at the spot a pile of ice a foot in height was made about it and then this covered with stones and gravel. A stake so planted remained perfectly firm and suitable for observations for some four days. The telescope was placed on the shore and protected from the sun. The two series of observations, carried on independently by each observer, were found to agree satisfactorily. The scales themselves were divided into half centimeters, but could be read by estimate to millimeters. As a control over the observations, and to prove that the movements were really those of the glacier and not of the rod itself, a second signal was planted in the ice near enough to the first to be included in the same field of view; it was found that the movements of both corresponded closely.

As examples of the results obtained, the measurements on September 3 and 4 are taken, the first was a perfectly clear and cloudless day, while on the second the sky was (especially in the morning) covered with cumuli, and the glacier only partially affected by the sun's rays. The results may be stated as follows: for scale I (35 meters from the edge of the glacier) the movement during the afternoon was slight, both horizontally and vertically, and this downward; later in the afternoon the movement diminished still more and finally ceased. During the night there was a movement of the point vertically upward and horizontally toward the valley. When the sun's rays first fell upon the ice, and from then to midday the movements were quite irregular, then the movement downward again began and the preceding effects were repeated.

The course of scale II, 90 meters from the edge of the glacier, was in part similar; that is, downward, horizontally and vertically, during the afternoon until 4 o'clock P. M., when it was stationary; on the other hand, during the night there was a strong backward movement (as much as 9·9 cm.), while vertically the movement was slight; from sunrise till midday as in the other case the movements were quite irregular. Similar results were obtained for another point situated about three-fifths of a mile higher up on the glacier.

The magnitude of the movements referred to will be seen from the following table, where the measurements are given in centimeters:

| Hours, P.M. | Scale I. | | Scale II. | | Hours, A.M. | Scale I. | | Scale II. | |
|---|---|---|---|---|---|---|---|---|---|
| | Horizontal | Vertical | Horizontal | Vertical | | Horizontal | Vertical | Horizontal | Vertical |
| 12 –12.30 | +1·0 | +0·6 | +0·6 | +0·2 | 6 – 7 | +1·1 | −0·1 | −0·1 | +0·3 |
| 12.30– 1 | −0·2 | ±0·0 | +0·5 | +0·3 | 7 – 7.30 | +0·6 | +0·7 | −2·1 | +0·2 |
| 1 – 1.30 | +0·1 | ±0·0 | +0·5 | +0·2 | 7.30– 8 | −0·7 | −0·3 | +1·3 | +0·8 |
| 1.30– 2 | +0·2 | +0·3 | +0·6 | +0·3 | 8 – 8.30 | +2·3 | +0·2 | +2·1 | −3·3 |
| 2 – 3 | +0·1 | +0·5 | +0·9 | +0·6 | 8.30– 9 | +0·7 | −0·3 | +1·7 | +3·5 |
| 3 – 4 | +0·1 | −0·1 | +0·3 | ±0·0 | 9 – 9.30 | −1·6 | +1·2 | +0·4 | +0·8 |
| 4 – 5 | −0·4 | +0·2 | ±0·0 | ±0·0 | 9.30–10 | −0·1 | +1·1 | +1·6 | +1·0 |
| 5 – 6 | +0·8 | −0·1 | −0·3 | ±0·0 | 10 –10.30 | +0·7 | +0·8 | +2·3 | +0·3 |
| 6 – 6A.M. | +2·3 | −1·0 | −6·2 | +1·0 | 10.30–11 | +1·8 | −0·4 | −0·9 | −1·0 |
| | | | | | 11 –11.30 | −0·8 | −0·2 | +0·6 | −0·2 |
| | | | | | 11.30–12 | −0·5 | +0·2 | +0·8 | +0·4 |

The observations on the following day correspond with those given except that the movements were less decided, connected, it is believed, with the less intense action of the sun's rays. The authors conclude that the movement of a glacier is far from being a uniform one; on the contrary, that one and the same point may have a motion now up, now down, both vertically and horizontally; further than this, that two points, at a distance of say 50 to 60 meters, may move at a different rate or even in different directions. They promise to carry on a similar series of observations, on a much larger scale, in the coming summer. These are in fact needed to establish the conclusions they draw, and to show that the observations have anything more than a local significance.— *Wiedemann's Annalen*, viii, 661.

2. *Volcanic action in Dominica.*—Dr. H. A. A. Nichols, who, we believe is Surveyor-general of Dominica, publishes in Nature (xxi, p. 372) an interesting letter describing an eruption in that island, shortly after 11 o'clock, A. M., on January 4. In the town of Roseau, the capital of the island, the phenomena consisted of a very heavy rain, accompanying a fall of light gray ashes, which covered the ground to the depth of a quarter of an inch. The Roseau river, which rises near the volcanic district, became a torrent of opaque white water. The scene of the eruption is about eight miles east of Roseau, and the ashes were blown westward by the tradewind, in a belt one and a half miles wide, and extending at least four miles out to sea. The crater

was visited on January 12, and was found to be an old one reopened in the forest about a mile southwest of the Boiling Lake, and to be some 600 feet deep. There was no earthquake, or at least none felt at Roseau. There was no flow of lava and not much evidence of fire, the eruption consisting of ashes and a gray mud, of which an enormous quantity was thrown out. The ashes, a pint measure of which without compression weighed 21 ounces and 15 drachms, were analyzed and found to contain ferric sulphide, magnesia, potash, soda, silicon, sulphur, carbon, oxides of iron, lead and alumina, with traces of other substances. C. G. R.

3. *The Microphone as a Seismometer.* (From a note in the Japan Gazette, quoted in Nature, xxi, p. 382.)—It appears that Professor MILNE of Tokio, has employed a special form of microphone to detect seismic tremors too slight to affect the ordinary seismometers. The similar use of the instrument by Professor M. di Rossi at Rome, two years ago, has already been noticed in this Journal, xviii, p. 159. Professor Milne's microphones, the special form of which is not described, were buried in pits round about the house and at a distance from roads, precaution being taken to exclude insects, which might disturb the sensitive apparatus. Under these circumstances, it would seem, that for sometime before an earthquake shock, the telephone gives signs of crackling in the earth as if it were exposed to an increasing strain, under which it finally gives way, and produces the vibration of the earthquake. C. G. R.

## OBITUARY.

The naturalist Wm. Ph. Schimper died at Paris on the 20th of March. This celebrated author was born at Dosenheim, a village of Alsatia, Jan., 1808. At the time of his death he was Professor of Geology and Director of the Museum at the University of Strasburg.

As botanist he first gave especial attention to the study of Mosses. With the coöperation of Ph. Bruck and Th. Gümbel, two of his countrymen interested in the same kind of researches, he began in 1836 the publication of the *Bryologia Europæa* which occupied twenty years of his life. This work, a grand scientific monument, contains in six quarto volumes a detailed description of all the species of Mosses known in Europe, each illustrated by a full platę of figures, beautifully and exactly reproducing the characters of the divers parts of the plants and of their variations.

This production gave Prof. Schimper an eminent and well-merited place in the scientific world, for it was mostly the result of his own labors; Bruck having died long before it came to an end, while the coöperation of Gümbel was only temporary.

As a compendium to the Bryologia Europæa, Schimper published, in 1857, a memoir on the history of the *Sphagnum*, a quarto volume with fourteen plates, splendid illustrations of the germination, the progress of development, and characters of these plants of the family which he separated from the Mosses on account of their derivation from a prothallus like the Lichens.

In 1860 he published, in Latin, a synopsis of the European Mosses, preceded by a history of Bryology, and an exposition of the general characters of this order of plants, with systematic tables, maps representing the geographical distribution of the species, and plates for illustrations of the genera. A second greatly enlarged edition of the work was published in 1876.

The museum of Strasburg was already, when Schimper became its director, very rich in specimens of fossil plants. A large portion of the materials published by Brongniart in his *Végétaux Fossiles* had been derived from it. Schimper had there favorable opportunities to satisfy his taste for vegetable paleontology, a branch which had been part of his study in the German universities. An intimate friend and collaborator of Hugo Mohl, Alex. Braun, and other German physiologists of celebrity, he had acquired great proficiency in the anatomy of the vegetable organs. Already, in 1840, he published, with the coöperation of A. Niougeot, a monograph of the fossil plants of the *Grès bigarré des Vosges*. Later, in 1862, another work of the same kind, on the *Végétaux des Terrains de Transition des Vosges*, was prepared with the assistance of Kœchlin Schumberg, who furnished the geological exposition.

These were mere preparations for a much larger undertaking —that of the publication of his *Traité de Paléontologie végétale*, which in three large volumes gives the descriptions in Latin, with remarks in French, of all the species of fossil plants known to the time when it was finished, 1874, together with a folio atlas of one hundred and ten plates for illustrations of the more interesting vegetable types. This work demanded for its preparation long researches in the museums and private collections of fossil plants in Europe, and a critical review of all that had been published in paleo-botany. As an inexhaustible mine of materials referable to vegetable paleontology, the book has become an indispensable assistant to students and collectors of fossil vegetable remains.

A new manual of Paleontology, written in German and distributed in two parts, one for the animals by Karl A. Zittel, the other for the plants by W. P. Schimper, is yet unfinished. The half volume prepared by this last author and already published reviews the Thallophytes, the Bryophytes, and the Pteridophytes.

This short record of Prof. W. P. Schimper's productions cannot give an idea of the amount of work performed by this celebrated naturalist, during a career harassed by the most distressing and disheartening circumstances. It says nothing of the noble character of the man, who, always genial, kind and obliging to every one who needed his assistance, has left, as sincere friends to deplore his loss, all those who had intercourse with him.

L. LESQUEREUX.

Description of twelve new fossil species and remarks upon others, by S. A. Miller. 16 pp. 8vo, with plates 9 and 10. Dec., 1879.

A Subject-Index of the Publications of the U. S. Naval Observatory, 1845–75; by Edward S. Holden. Prof. Math. U. S. N. 74 pp. 4to. Washington, 1879.

Archives de Biologie, redigées par M. Ed. Van Beneden et Ch. Van Bambeke. 1st year, 1880. In quarterly numbers, making a volume annually of about 600 pages and 20 to 25 plates. Subscription price 30 francs, or for single numbers 9 francs. Paris: G. Masson, éditeur.

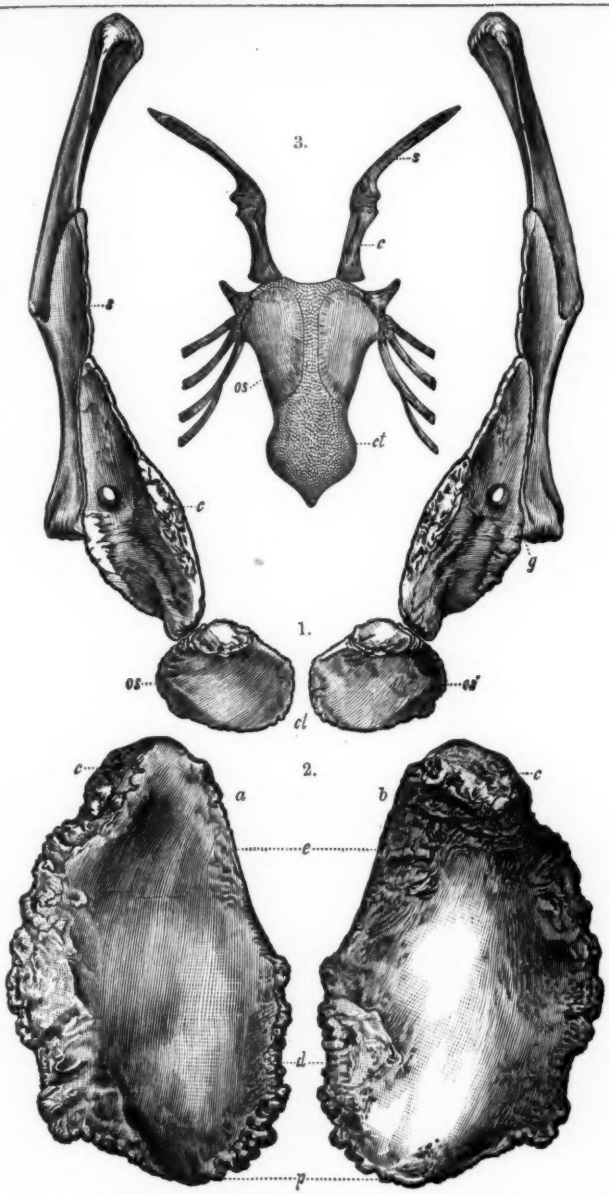


Figure 1.—Scapular arch of *Brontosaurus excelsus*, Marsh; front view, one-sixteenth natural size; *s.* scapula; *c.* coracoid; *g.* glenoid cavity; *os.* right sternal bone; *os'.* left sternal bone; *ct.* cartilage.

Figure 2.—Left sternal bone; one-eighth natural size; *a.* superior view; *b.* inferior view; *c.* face for coracoid; *d.* margin next to median line; *e.* inner front margin; *p.* posterior end.

Figure 3.—Scapular arch of young *Rhea Americana*, Lath.; (after Parker); three-fourths natural size; seen from below. Letters as above.